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TRANSACTIONS

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AMERICAN PHILOSOPHICAL SOCIETY.

NEW SERIES.

No. I.

Experiments to determine the comparative quantities of Heat evolved in the combustion of the principal varieties of Wood and Coal used in the United States, for Fuel; and, also, to determine the comparative quantities of Heat lost by the ordinary apparatus made use of for their combustion. By MARCUS BULL.—*Read April* 7, 1826.

THE experiments on fuel detailed in the following paper, were commenced in November, 1823, and were prosecuted with very little cessation, until June, 1824; when, in consequence of absence, together with subsequent ill health, they were suspended until May, 1825, when they were again resumed with undiminished interest, and have been continued, as circumstances would permit, from that period to the present.

During the latter of these periods, I was under the necessity of repeating those experiments which had been previously made, in consequence of a defect discovered in a part of the apparatus, the removal of which, was found to change the results; still, it was very satisfactory to find that the variation

was, in every instance, directly proportional to the results which had been formerly obtained.

The experiments to determine the comparative loss of heat sustained by using apparatus of different constructions for the combustion of fuel, appeared to be equally necessary with those to determine its comparative efficiency.

To Professors Hare and Patterson of the University of Pennsylvania, I am under obligations for their kind assistance in my experiments, and it gives me great pleasure to have an opportunity thus publicly to tender them my acknowledgments.

The importance of those experiments, which have for their object the promotion of the useful arts and sciences, or an improvement in the domestic economy of society, by which our comforts may be increased, is generally admitted.

In a climate like that of the United States, where, during two-thirds of the year, fires are indispensable to human comfort, and where, consequently, the savings of a large portion of the poor, during the summer, are often inadequate to purchase a sufficient supply of fuel for the winter; it must, obviously, be highly important to ascertain, the comparative efficiency of different kinds of fuel; as, without this knowledge, those who are desirous of economising, may be prodigal through ignorance.

The knowledge of the comparative heat disengaged in the combustion of the different varieties of wood and coal, is also important in various processes in the arts, and it is believed that the results of my experiments will be found worthy of attention, in a philosophical point of view.

Previous to describing my apparatus or experiments, it will be proper to notice those of some of my predecessors, as, in the investigation of this subject, no small degree of inaccuracy appears to have prevailed, even among experimenters of high character.

My remarks cannot be better prefaced, than by making use of the following extract from Dr. Ure, on the subject of combustion.

"Lavoisier, Crawford, Dalton, and Rumford, in succession, made experiments to determine the quantity of heat evolved in the combustion of various bodies. The apparatus used by the last was perfectly simple, and perhaps the most precise of the whole. The heat was conducted by flattened pipes of metal, into the heart of a body of water, and was measured by the temperature imparted."

From the general table of results, it is only necessary for me to extract two, to show the force of the succeeding remark.

Substances burned, one pound.	Ice melted in pounds.			
	Lavoisier.	Crawford.	Dalton.	Rumford.
Olive oil.	149	89	104	94.07
Charcoal.	96.5	69	40	

"The discrepancies in the preceding table, are sufficient to show the necessity of new experiments on the subject."

As the experiments of M. Lavoisier, Dr. Crawford, and Mr. Dalton, did not comprise any article of fuel except charcoal, a more particular notice of them would be irrelevant to my purpose.

The experiments of Count Rumford, to determine the quantity of heat evolved in the combustion of different woods, will alone be examined. In his very just remarks, he says, "Many persons have already endeavoured to determine the relative quantities of heat furnished by wood and charcoal in their combustion; but the results of their inquiries have not been satisfactory.

Their apparatus has been too imperfect, not to leave vast incertitude in the conclusions drawn from their investigations. Indeed, the subject is so intricate in itself, that with the best instruments, the utmost care is requisite, lest, after much labour, the inquirer should be forced to content himself with approximations instead of accurate results, and valuations, strictly determined.

All woods contain much moisture, even when apparently very dry; and, as the persons alluded to have neglected to determine the quantities of absolutely dry wood, burned by them, much uncertainty prevails in the results of all their experiments. Another source of uncertainty, lies in the great quantity of heat suffered to escape with the smoke and other products of the combustion."* Again,† "attempts have been long ago made, to measure the heat that is developed in the combustion of inflammable substances; but the results of the experiments have been so contradictory, and the methods employed so little calculated to inspire confidence, that the undertaking is justly considered as very little advanced. I had attempted it at three different times within these twenty years, but without success. After having made a great number of experiments with the most scrupulous care, with apparatus on which I had long reflected, and afterwards caused to be executed by skilful workmen, I had found nothing, however, that appeared to me sufficiently decisive to deserve to be made public. A large apparatus in copper, more than twelve feet long, which I had made at Munich fifteen years ago, and another scarcely less expensive, made at Paris four years ago, which I have still in my laboratory, attest the desire I have long entertained, of finding the means of elucidating a question that has always appeared to me of great importance, both with regard to the sciences and to the arts. At length, however, I have the satisfaction of announcing to the class, that, after all my fruitless attempts, I have discovered a very simple method of measuring the heat manifested in combustion, and, this even with such precision, as leaves nothing to be desired."

It will not be necessary to describe the *Calorimeter* used by Count Rumford, more particularly, than to say, that it consists of a small copper receiver containing water. In the inside is a flat worm, also made of copper, bent so as to pass horizontally three times from one end of the receiver to the other. This worm passes down through an aperture in the bottom,

* Nicholson's Journal, XXXV. 105.
† Ibid. XXXII. 105.

near one end of the receiver, to which it is soldered; and the other extremity of the worm passes through the opposite end of the receiver. A thermometer is introduced into the water contained in the receiver; the woods, in thick shavings, and other combustible bodies, are consumed in the mouth or bottom of the worm, and the heat evolved in the combustion, is imparted to the water during its passage through the worm.

The experiments consisted in elevating the temperature of the water in the receiver 10°, commencing at 5° below, and finishing at 5° above the temperature of the room; and the comparison was made between the weights of different articles required to be consumed to produce this effect, *without regard to time.*

The quantity of wood consumed, varied from 59 to 111 grains in each experiment.

Upon these experiments it is necessary to remark, that the passage of the mercury from 1 to 10° on the scale of the thermometer, can scarcely be supposed to have been performed in all the experiments *in equal periods of time;* and, since the water would require unequal increments of heat in equal times, to counterbalance its unequal decrements, and, possessing, as it does, different capacities for heat at different temperatures, consequently, a very slight inequality in point of time, in elevating the mercury between the several degrees, would materially affect the results of experiments in which only a few grains of the combustible were consumed.

To these causes, and the absence of proper means to take advantage of the heat produced in the combustion of the carbon contained in the woods, may be attributed the inaccuracy of Count Rumford's results; as he states some of the woods to evolve, by the combustion of equal weights, 64 per cent. more heat than others; whereas, the results of my experiments on forty-six varieties of wood, in equal weights, give the extremes of difference as only 11 per cent.

The result from charcoal is not given in the table, but the says, that "The dry vegetable flesh of wood, produces more

heat in its combustion, than an equal weight of dry charcoal."*

By the expression "dry vegetable flesh," the count means to indicate that portion of dry wood which is inflammable, or that part which is independent of the charcoal. Now I find, by the most favourable comparison for this portion of the wood, that an equal weight of dry charcoal, produces 286 per cent. more heat than the former, and by the least favourable comparison, 314 per cent. more, giving a mean difference of 300 per cent. in favour of the charcoal.

It will be proper to state what has been considered as essential requisites to the perfection of the apparatus, that, as the description proceeds, the degree of accuracy which it is likely to possess, may, with greater facility be determined; and this will be done under three heads, with explanatory remarks.

1st. *That the apparatus in which the combustion is produced, be so constructed, that all, or an equal proportion of all the heat generated, may be measured by some unchanging standard.*

This is effected in a manner to be hereafter more particularly described, but it may now be sufficiently understood, by referring to the plate, in which the apparatus and the interior of the room, constructed for performing the experiments, are shown in perspective. At E is a thermometer, the bulb of which is in the centre of the stove-pipe, and another, Fig. 6, is suspended from the side wall of the room.

When articles are submitted to combustion in the stove, the heat is so completely given out by the pipe, that these two thermometers, indicate *exactly* the same degree of temperature.

Strictly speaking, we cannot say even in this case, that *all* the heat generated is imparted to the air of the room. That small portion which is included in the air of the pipe, and passes off into the chimney, does not impart its heat to the air of the room, both being of the same temperature, consequently, no

* Nicholson's Journal, XXXV. 112.

interchange of heat can take place between them. We may consider this escape of heat, however, in the same point of light as we do that which is conducted off by the surface of every other part of the room, with this difference—that *this particular surface of two inches diameter, conveys more heat in a given time, than any other equal surface;* but as this difference is uniform in all the experiments, we may say, *comparatively*, that there is *no loss* of heat, as it is the *ratio*, and not the *positive* quantity of heat disengaged, which we wish to discover.

2d. *That the recipient body be always affected equally by the communication of the same heat.*

Air has been selected as the recipient body, because we are enabled by a thermometer to measure with accuracy the heat communicated to it; and because it varies very little in its specific heat, under the ordinary changes of barometric pressure, and its hygrometric changes may be readily counteracted.

3. *That the surrounding refrigerating medium be permanent at any required temperature.*

In consequence of the variations in the temperature of the atmosphere, not only daily, but in different parts of the same day, to devise a plan which should strictly comply with this requisition, was a subject which caused me much reflection and perplexity. The room selected for my experiments, was well calculated, in every respect, (except the window,) to prevent an immediate influence being produced in its temperature, by the ordinary external changes. The window being large, I determined to close it entirely, and to perform my experiments by lamp light, and it was, accordingly, perfectly closed on the inside of the room, with boards, which were well seasoned, and grooved together, leaving a space of four inches between this barricade and the sashes of the window. This space being occupied with *confined air*, was a bad conductor of heat. Finding it inconvenient, and objectionable in other respects, to experiment with artificial light, a sash with four panes of glass was subsequently inserted in this barricade, for

the admission of light. Every part of the room was then made as tight as possible, and to furnish the room with the necessary supply of air, of equal temperature, a pipe with a valve was inserted through a partition into an adjoining room, as its temperature was necessarily maintained very uniform, for the purposes to which it was applied. Having spent nearly four months of application in perfecting my apparatus, and removing difficulties which presented themselves at the threshold of every stage of the investigation, and feeling desirous to avail myself of any improvements which might be suggested to me, either in the apparatus, or the intended plan of conducting the experiments, I invited several gentlemen to examine it for that purpose, and among them, Dr. Hare, professor of chemistry in the University of Pennsylvania.

The method which had been adopted, as described, to comply with the last requisition, did not appear to Dr. Hare to possess that degree of accuracy which was necessary, nor did it equal that which every other part of the apparatus, together with the intended plan of conducting the experiments, as described to him, appeared to possess. Dr. Hare stated to me, that, "he had long been under the impression, that no accurate comparison could be made by means of the same single room heated at different times, with different fuel, on account of the varying temperature of the weather; nor by different rooms at the same time, from the difficulty of finding two rooms sufficiently alike, in form, aspect, size, and materials. It seemed to him indispensable, to have one room within another, so that, in the interval, a uniformity of temperature might be artificially sustained." As the method suggested by Dr. Hare, removed this difficulty with which I had unsuccessfully contended, no time was lost in making a practical application of his suggestion, and a room of smaller dimensions was in consequence constructed within the room originally intended for my experiments, in the best manner which my architect could devise; by which a free circulation of air is produced on all the exterior surfaces of the interior room, and this air may be sustained of a uniform temperature.

A description of the apparatus, plan of the experiments, and the manner of experimenting, will now be detailed.

In a room with a floor of about eleven feet by fourteen, and nine and a half feet in height, another room is constructed, eight feet square in the clear, its contents being 512 cubic feet. The plate represents the interior of this room in perspective, and as these rooms may now be considered as distinct, I shall, for convenience, designate them by the names of *interior* and *exterior*.

The frame of the interior room is composed of scantling, three inches by four. The ends of the posts, and top and bottom rails, have mortises, with tenons passing through them, of sufficient length to project about four inches, and, in the projecting part of the tenons, are transverse mortises for wedges, by which the frame is drawn firmly together. The floor is supported by two cross pieces of scantling, and the posts and rails are grooved through the centre, to receive boards one inch in the clear, with which the room is enclosed. The boards are also grooved together in the most perfect manner, so that the wedges (there being no nails used except about the door and window) will draw every part of the room tight, and correct, with great facility, any shrinking of the boards during the process of seasoning, which it was necessary to perfect, previous to any experiments being made.

The interior is supported by its four posts, six inches from the floor of the exterior room, there being the same distance between the ceilings, and a much greater between the side walls, the air therefore circulates freely between the two rooms. The internal surfaces of the interior room are made as white as possible with lime-wash, to produce equality in their power of conducting heat. The body of the stove, Fig. 1., is a cylinder, twelve inches in height, and four inches diameter; the ash pit is four inches deep, and four inches in diameter; both are made of common sheet iron, and separate, for the purpose of introducing between them, a chamber, or concave piece of sheet iron, of larger dimensions, perforated with holes half an inch in diameter; and on this chamber the body of the stove

rests, as will be seen, by referring to the enlarged sectional view on the plate, Fig. 2. Three inches above this chamber is another, closely fitted within the body of the stove, and perforated with holes one quarter of an inch in diameter. The interior of the body of the stove above, is made to assume the conical shape which it presents, with the apex downwards, by coating it with fire clay, so as to expose only one and a half inches diameter of the surface of the chamber, and on which the fuel rests. The space between the chambers is necessary in experimenting on anthracite coals in small quantities, for the purpose of heating the air as much as possible before it comes in contact with the burning body, and the clay coating is also necessary in the same experiments, to act as a non-conductor. The stove, Fig. 1., is supplied with air through apertures just above the ash pit, or lower door, and to lessen, or close these apertures, a sliding sheet iron hoop, (not shown in the engraving,) is fitted with great accuracy. The middle door is necessary, to obtain access to the upper chamber when its apertures require clearing, during an experiment. For heating water, a tin vessel in the shape of a crescent, rests on cleats, between the upper and middle doors. This vessel is accurately fitted to the body of the stove, but may be removed to any required distance, at pleasure; and we may thereby lessen the evaporation of the water, its object being to regulate the hygrometric state of the air.

All the doors of the stove are represented as open. The upper door is to admit the fuel. The cone, leading from the body of the stove to the pipe, is ten inches long, and very accurately fitted to the former, but removable for the purpose of separating them, to take from the stove and ash pit, the unconsumed parts of any body, that may have been experimented upon. This is done with facility, as the pipe is supported from the ceiling, by wires which sustain it in its place, after the body of the stove is removed.

In the cone, three quarters of an inch above its junction with the body of the stove, (which in this place is made flat,) is an aperture one inch broad, and one and a quarter inches

long, which is covered with a thin plate of mica, resting on a flange, or ledge, and kept in its place by a wire passing round the cone. Through this plate of mica, the fire may be seen, thereby avoiding the necessity of opening the upper door for the purpose of mere examination.

The pipe is two inches diameter, and made of extra thin *black* tin, to impart the heat to the air of the room with the least possible obstruction. The elbow joints are each nine inches long. The whole length of the pipe is forty-two feet; and this was found insufficient to impart to the air of the room all the heat generated, there being a loss of 3°, until the tin box, A, was attached to the pipe near its extremity. This box is fourteen inches long, ten inches broad, and $\frac{3}{8}$ths of an inch deep, and its interior and exterior surfaces are made black. In passing through this box, the warm air is exposed to a much larger surface than that presented by the pipe, and the few degrees of heat which it before contained, are by this means imparted to the air of the room.

The joints of the pipe are perfectly closed by clay lute, and its whole exterior surface is covered with a thin coat of dead black varnish, made to resist heat.

The valves B, C, D, to regulate the admission of air into the stove, are all of the same construction, being circular pieces of flat thick sheet iron, very accurately adjusted, to close the interior of the pipe. Fig. 3, represents a side view of the valve B, standing entirely open. The wire to which it is firmly riveted, crosses the centre of the valve, and passes through the pipe. This end of the wire serves as one of the pivots for the valve to turn upon, and the other end, being bent into a half circle, is used both as the handle to turn the valve, and as an index to regulate it. The point of this enters the graduated holes in the dial; Fig. 4, which is a front view, and is riveted to the exterior of the pipe, being the half of a circle of flat sheet iron, whose whole diameter is equal to that of the pipe. The handle is bent to correspond exactly with the flat surface of the valve, by which the situation of the handle indicates the position of the valve inside of the pipe, so that no mistake can occur in its use.

Being well aware that the experiments could not be accurately performed, unless the operator should at all times possess a perfect control over the burning body; it became necessary after attaching the box A, to insert the cross pipe with the valve D, by which the current of air through the stove may, in an instant, be placed at its maximum in quantity and velocity, if permitted to pass through this cross pipe, in place of passing through the shallow box A.

This passage is useful when igniting anthracite coal, in which process, the coal, as well as all other combustible bodies, require to be heated to a certain temperature before they will ignite, during which process, heat being absorbed, and not disengaged, if care be taken to close this valve in proper time, none is lost. As this required temperature differs not only in different bodies, and in the different component parts of some bodies, but is specific, for each, it may for convenience, be termed their *heat of ignition* or *accension.*

This passage is also useful in some experiments, to give a momentary impulse to the inflammation, of certain bodies, and cannot be dispensed with without great loss of time, in heating the room to its proper temperature, before commencing an experiment.

Considerable difficulty was experienced in getting the valves and their appendages made with sufficient accuracy, but when done, as half of the arc of each dial is divided into twenty equal parts, it will be perceived that the current of air to supply the body in combustion, can be regulated with great precision.

The valve B, is particularly useful to stop at a proper time the combustion of those bodies, which it is known cannot be wholly consumed in the stove, and this is done almost instantaneously by closing this valve, and sliding down the hoop which covers the apertures for the admission of air.

The pipe passes through the side wall into the chimney of the exterior room. Near the end of the pipe, within the interior room, is an aperture of sufficient size to admit the bulb of the thermometer E, and this aperture is closed by a tin plate closely fitted to the stem of the thermometer. This plate is

curved to fit the pipe, and is of sufficient size to cover the aperture, and rest upon the pipe. The bulb of the thermometer is suspended in the centre of the pipe, by the brass scale being made shorter than usual, and resting on the tin plate, which is secured in its place by a small quantity of clay lute. This thermometer is used to measure the temperature of the air within the pipe, previous to its passing into the chimney; and as I have never found the bulb discoloured by the carbonaceous particles in the smoke, and thereby rendered more sensible, as it was feared would be the case, I am induced to think very little ever reaches it, being previously deposited in the pipe.

Fig. 6, is another mercurial thermometer, suspended from the side wall of the room. Both these were made expressly for my experiments, and to correspond in their scales (which are Fahrenheit's) with the greatest possible accuracy. The thermometer, Fig. 6, is used to measure the temperature of the air in the room, and is placed on a line with that in the pipe, at twelve inches distance. The bulb is screened by a piece of bright planished tin, to prevent the influence of heat radiated from any part of the stove or pipe, while it does not prevent a free access of the air in the room, to the bulb of the thermometer.

Fig. 7, is Mr. Leslie's differential thermometer, one half of which is passed through an aperture in the board partition into the exterior room, and is secured in its place by a divided cork, which encircles a part of the syphon at the bottom of the instrument, and closes the aperture. Both bulbs are perfectly screened by large pieces of bright planished tin, not shown in the engraving. This instrument, as its name denotes, measures only the *difference* of temperature in the two rooms, and as it does this with peculiar delicacy, it is admirably adapted to my purpose, the accuracy of my experiments depending in a great measure on the uniform difference of temperature in the two rooms; and I am under obligations to its inventor, and also, to Dr. Hare, as it was in consequence of the suggestion of the latter gentleman, that this instrument was added to my

apparatus; its peculiar applicability to my experiments not having previously occurred to me.

The differential thermometer used in my experiments, indicated 20°, to 1° of the mercurial thermometers, and, as one of the bulbs is situated in the interior room, it can only be operated upon by the temperature of that room; the other bulb being in the exterior room, can only be operated upon by the temperature of the latter room; consequently, any change of temperature in either will be shown on the scale, the instrument having been adjusted with great care, so that the top of the tinged liquor will stand at 50°, when there is a difference of 10° between the mercurial thermometers placed in the two rooms; and from its superior sensibility in detecting incipient changes, the differential thermometer may almost be said to possess the power of *divination*, whereby the operator receives timely notice to avoid any essential error.

Fig. 5, is a tin supply pipe, two inches in diameter. This passes through the floor in a perpendicular direction, and has an elbow joint opening towards the stove. It has a valve to regulate the quantity of air found necessary to be admitted into the room for the purposes of respiration, and to support the combustion in the stove. This valve, when once adjusted, remained the same through all the experiments. Whether the precise quantity of air necessary for the respiration of the operator, and to support the combustion, is admitted by this pipe, *or an excess*, its temperature being the same, and the stove being supposed always to be supplied with air at the temperature of the interior room, and to require about the same quantity during any given period of two or more experiments, the air admitted being also of equal volume, its velocity will be the same under all changes of barometric pressure; consequently, the reduction of the temperature of the air in the room may be supposed to be the same during the time required to perform each experiment, with the exception of an immaterial variation in its specific heat, to be hereafter noticed; and, the results of the experiments cannot be affected by the admission of an excess of air, they being, as before stated, founded on the comparative, and not the positive quantity of heat evolved.

At Fig. 8, is a hygrometer made of the beard of the wild oat, enclosed in a small brass case, and covered with glass. This is used to measure the humidity of the air, which, like all other bodies, possesses different conducting powers as its hygrometric state varies, by which its specific heat or capacity for absorbing caloric is increased or diminished; those bodies which contain moisture being better conductors than the same bodies when dry. The comparative capacities of water and dry air, are, as 1.000 to .266, by the experiments of MM. Delaroche and Berard. From Sausseur's experiments, it appears, however, that the quantity of aqueous vapour attracted by the air of the atmosphere, when at 65° of Fahrenheit, is very small; a cubic foot of air requiring not more than eleven or twelve grains to bring it from the state of perfect dryness, to that of extreme moisture.

Now, as the various sides of the room are the conducting media by which the heat generated in the room is dissipated, and as these sides are in contact with the air of the room, and must in some degree be influenced by its hygrometric state, they will, consequently, become more or less powerful conductors, as this varies. To produce a uniformity in this respect, I have, by the aid of this instrument, and of the water contained in the tin vessel before described, taken care to keep the air of the interior room in the same hygrometric state, during the various experiments.

The barometer at Fig. 9, requires no description, and is not considered an essential appendage to my apparatus, although convenient as a check upon the valves; not, however, on the common supposition that the velocity of the current of air through the stove is greater under one pressure than another, *cæteris paribus*, but that its quantity varies with its density, more being contained in the same volume at one pressure than at another.

The results of MM. Clement and Desormes' experiments on gases, to determine their *specific heats*, at different densities, show that the specific heat of atmospheric air does not vary more than .02, between 29.5 and 30.5 inches of barometric

pressure. These being the extremes during my experiments, the difference of heat required to maintain the temperature of the air between any two experiments, cannot materially affect their results, and for this variation no correction has been thought necessary.

Having described the construction of the interior room, and its apparatus, it remains to describe the exterior room, which has a capacity of 860 cubic feet, after deducting 542 feet for the space occupied by the interior room, and the materials of which it is composed. This room has a southern aspect, and is defended from the west winds by a building projecting beyond it ten feet south. It has one window, with blinds on the outside, to exclude, when necessary, the rays of the sun; the east and south walls are of brick, and are ten inches in thickness; the remaining two are partitions of lath and plaster, four and a half inches thick, and separate between a passage on the west, and a room on the north. The chimney is in the east wall. A small stove is placed in this room, the pipe of which passes through the fire-board. A mercurial thermometer, to measure the temperature of the air, is placed in a convenient situation, on a line with those in the interior room, and on a table an accurate balance is suspended, to weigh the articles which are to be subjected to experiment.

The plan of the experiments will next be described.

Equal quantities are taken of each article by weight, previously made absolutely dry; by which is to be understood, that state of deprivation of moisture manifested when no diminution in weight can be effected by the heat of a stove at 250° of Fahrenheit.

It is required to determine the period of *time* which the combustion of each article will maintain the temperature of the *interior* room 10° higher than the *exterior;* and the time that the interior room is thus maintained by any article, gives its true relative heat, when compared with the time which any other article has maintained the room at the same difference of temperature. As the temperature of the air in both rooms is supposed to remain *stationary*, the increments and decrements

of heat will therefore be equal, in equal periods of time, in all the experiments, by which the objections made against the *plan* of Count Rumford's experiments are considered as obviated.

The manner of experimenting is as follows:

The first step to be taken by the operator, is to produce the required difference of 10° between the interior and exterior rooms, and to arrange the necessary coincident circumstances for its perpetuation.

As no artificial refrigerating means can, with convenience, be made use of to depress the temperature of the exterior room below that of the atmosphere, it becomes necessary that the temperature of this room shall, in the first instance, be higher than any elevation which will occur in the temperature of the atmosphere during an experiment, otherwise the experiment must fail.

During the many trials of the apparatus in order to become familiar with its use, and to lessen the great difficulty experienced in maintaining the uniform difference of temperature required between the interior and exterior rooms, the following incident occurred, by which this difficulty was entirely obviated.

In the month of June, an unusual depression in the temperature of the atmosphere had taken place during the night season, in consequence of which the temperature of the exterior room was found on the following morning to be 20° above that of the atmosphere. Having been previously obliged to experiment at very high and uncomfortable temperatures, in consequence of the heat of the weather, and presuming that this depression would be transient, and as my assistant, who attended to the exterior room, was absent, no increase was made in its temperature, as had formerly been done under similar circumstances. The temperature of the interior room was elevated, without previous calculation, 15° above that of the exterior room, at the period of commencement; during this operation, the thermometer in the exterior room had not been observed, but on examination, the difference was found to be precisely 10° between the two rooms; considering it, however, as a fortuitous

occurrence, no investigation of the cause was at that time entered into. The trial experiment was commenced under a firm belief that the differential thermometer would give immediate notice that the temperature of the exterior room required correction, but, to my astonishment, the differential thermometer was found to vary less than usual, and after a lapse of three hours, although the temperature of the atmosphere was found to have been elevated 12°, the temperature of the exterior room remained *stationary*, and continued so until the completion of the experiment.

No time was then lost in attempting to discover the cause by which an effect so desirable had been produced, and when examined, it became a matter of surprise that it had not previously been discovered by calculation and experiment, rather than accident. It may be explained in the following manner:

The interior room contains 512, and the exterior 860 cubic feet of air. As the heat necessary to elevate 512 cubic feet of air 15°, is gradually transferred to 860 cubic feet, consequently, it must increase its temperature so long as its increments are greater than its decrements, and should, by calculation, *cæteris paribus*, augment it nearly 9°, instead of 5°, as was found to be the case; but as the exterior room presents very nearly double the conducting surface, this will account for the difference.

When the temperature of the interior room is thus elevated 15°, the exterior is consequently elevated 5°, by which the required difference of 10° is produced, and the temperature of the exterior room then becomes *stationary*, that being the precise point at which the increments and decrements of heat are equal in the air of *both rooms*.

The manner of producing this important result under known circumstances, being established, the operator has only to seek for the same result in a different place, under an unknown, or known difference of circumstances. As the surface of the window (the barricade having been removed) is the only part of the exterior room which can be speedily operated upon by the ordinary changes of the atmosphere, the temperature of

the room, must therefore, from its situation, and the nature of its walls, change very little; if, however, during an experiment, any indication of an increase in its temperature is observed, the upper sash in the window, which is suspended with weights, is lowered the required distance to correct it; but if decreasing, a fire of wood can be immediately kindled in the stove, a lamp being kept burning in this room for the purpose, although never required but in two instances during my experiments.

The required difference of temperature between the two rooms being adjusted as described, it is maintained for about half an hour by burning dry charcoal. The article to be subjected to experiment is then accurately weighed, and if it is wood, the unconsumed charcoal is wholly removed from the stove by a small pair of tongs, and deposited in another room, and the wood which is used in pieces two inches long, and half to one quarter of an inch thick, is ignited by applying it to the flame of a lamp; but if it is any of the species of coal which cannot be ignited *per se*, the burning charcoal is taken from the stove and weighed, and its quantity either increased or diminished so as to make half an ounce, which is quickly returned to the stove, and on my notes, the name of the article, its quantity, and the time, by an accurate watch, are then set down, together with the state of the thermometers, the barometer, and hygrometer. The heights of the thermometers are noted every ten minutes during the experiment, that in the exterior room being always known by comparing the mercurial and differential thermometers of the interior room.

The last ten minutes of time which is entered to finish an experiment, is that to which it approaches the nearest; the difference therefore from the proper time, cannot be more, but will generally be less than five minutes, which is, in many cases, as near perhaps as it can be determined, and the greatest difference stated will not affect the mean of the results one per cent.

The anthracite coal cannot be wholly consumed, even in the improved state of the stove, the upper chamber having been

introduced after its first construction, to provide a space for the purpose of heating the air as much as possible before coming in contact with the burning body, by which the quantity remaining unconsumed is reduced from two ounces to less than half an ounce. That portion which remains unconsumed after an experiment, including the small particles which drop through the apertures of the chambers into the ash pit, are washed upon a sieve to remove the ashes and any other foreign matter, and when thoroughly dried in a crucible, are weighed and deducted from the original weight.

In making up the results of experiments in which charcoal is used to ignite the body, from the resulting time is deducted so much as is known by previous experiment to have arisen from a portion of charcoal equal in weight to that used. Those bituminous coals which fuse and cake in the process of coaking, are the most troublesome to manage in small quantities, from the inconstant manner in which the bituminous part burns, and its tendency to become extinguished the moment that portion is consumed; the combustion of the bitumen not producing the heat of ignition required by the carbonaceous part to continue the process of combustion, and the surface being partially covered with the deposite from the pyrites, becomes more difficult to ignite, and requires to be broken asunder to present a fresh surface. To overcome this difficulty, it was found necessary to use the coal in very small pieces, and occasionally to take from the stove such parts as had coaked, break them in pieces, and return them to the stove as required, which, when ignited, will burn permanently, and the heat required to coak the remaining part of the coal is thereby produced. During tedious experiments, the operator is sometimes under the necessity of passing from the interior to the exterior room, but if done with proper caution, the differential thermometer is never affected thereby.

The animal heat imparted to the air of the room by the operator, must be noticed. This, under ordinary exertion of the muscles, being equal both in temperature and quantity, as determined by Dr. Crawford, and being the same during the

period of each experiment, the results will not be affected thereby.

The accuracy with which the experiments have been performed, is a delicate subject for me to expatiate upon, but I shall be permitted to say, that all means within my power have been used to render the results as accurate as the difficult nature of the subject will admit. These results will be found in the general table.

From the diversity in these results, it is apparent, that equal weights of different combustible bodies vary materially in the quantity of heat disengaged in their combustion. The woods differ less perhaps in *equal weights* than has been generally supposed, and that difference will be found to correspond very nearly with the different quantities of carbon they contain; they are however of very different value in *equal quantities by measure*, in consequence of the great disparity in their *weight*. This remark is also applicable to those coals which are sold by measure and not by weight, from which circumstance, it becomes necessary to *caution* those who would attempt to ascertain the value of different articles of fuel by merely comparing their different results of heat in the table, without regard to their different weights. The results being comparisons between articles in equal weights, cannot be compared with quantities by measure alone; hence the necessity of determining the weights of a given bulk of those articles sold in this manner, which will be found in the table in their respective columns, the manner of obtaining which will be hereafter detailed. The object of my experiments being practical utility, rather than scientific research, to facilitate the accomplishment of that desirable object, I have estimated the comparative values of the different articles. These will be found in the last column of the table, and are equally applicable not only to every market, but for every change in the prices that can take place.

The standard taken is shell-bark hickory, that being of greater weight than a cord of any other wood in the table, and disengaging in its combustion an equal quantity of heat from any given weight.

The comparative numbers express the value of one cord of each of the woods, one ton of the anthracite coals, and one hundred bushels of the bituminous coals, charcoal and coak, and although no one market is supposed to furnish for fuel every kind of wood contained in the table, yet the principal part will probably be found, and in markets where the woods are much mixed, averages may easily be made adapted to those markets. The column of comparative values was found in the following manner.

The value of a given quantity of fuel is directly proportional to the *time* that a given weight of it maintained the air of the room at a given temperature, and also to its *weight*. Hence assuming shell-bark hickory for a standard, since one pound of this wood maintained the air of the room at the given temperature 400 minutes, this being multiplied by 4469, the weight of a cord of this wood, we obtain 1787600 minutes as the time which the air of the room would have been maintained at the given temperature, by consuming one cord of this wood.

We then have the following proportion. As the product in time corresponding to one cord of shell-bark hickory, (1787600) is to its assumed value (100) so is the product of the weight of a given quantity of any other article into the time that one pound of it would maintain the air of the room at the given temperature, to the value of the given quantity of this article.

Thus for a cord of white ash wood:

$$\text{As } 1787600 : 100 :: 3450 \times 400 = 138000000 : 77$$

For a ton of Lehigh coal, of 2240 pounds:

$$\text{As } 1787600 : 100 :: 2240 \times 790 = 176960000 : 99$$

For 100 bushels of cannel coal weighing 6525 pounds:

$$\text{As } 1787600 : 100 :: 6525 \times 630 = 411075000 : 230$$

A few examples will be sufficient to show the facility with which the comparisons may be made. For this purpose, we will assume the price of shell-bark hickory wood as at six dollars for a cord of 128 cubic feet, this being the average price

in this market, and compare it with a cord of red-heart hickory. The comparative value of the former is 100, and of the latter 81. We then have the following statement. As 100 : 600 :: 81 : 486. Four dollars and eighty-six cents being the comparative value of a cord of red-heart hickory, and the difference between the price of this wood and its comparative value thus ascertained, shows how much dearer or cheaper it is than the wood with which it has been compared. We will suppose the price of red-heart hickory to be 5.75 and that of chesnut white oak to be 5 dollars. Then 81 : 575 :: 86 : 610, is the value of the latter, which being sold at 5 dollars, is cheaper by one dollar and ten cents, than the red-heart hickory. If we take the mean of the comparative numbers for the eleven different species of oaks, which is 69, and compare them at 5 dollars, with shell-bark hickory at 6 dollars, 100 : 600 :: 69 : 414, is the average value of those oaks, and at the prices specified, the hickory is the cheapest by nearly one dollar.

A mere examination of the comparative numbers, will show that a cord of white birch is 52 pr. ct. less in value than a cord of shell-bark hickory, and the difference *per cent.* may be calculated from the comparative numbers between any two articles sold at the same price.

We will now extend the comparison to some of the coals: and take for this purpose one cord of shell-bark hickory, at 6 dollars, and determine the comparative value of one ton of Lehigh Coal. As 100 : 600 :: 99 : 594, which shows them to be of nearly the same value, supposing each article to be consumed under the same circumstances; but as this is not the case, and as this objection has been frequently stated to me by those who have confounded two distinct subjects, a momentary digression will be excused, to show the futility and irrelevancy of this objection. It is admitted that there may be greater disparity between the manner of consuming different kinds of fuel, than actually exists in their comparative value as usually sold; but this difference does not enhance or depress the value of the different articles, provided it is practicable to consume them in the same manner, which, with very few exceptions,

may be done. The intrinsic value of the different kinds of fuel, and the loss or gain experienced by the different constructions of the apparatus used for their combustion, are distinct subjects of inquiry, and although both are necessary to be known, to effect any valuable improvement in the selection of the one and the construction of the other, yet it does not follow as a consequence, because the construction of a grate used for the combustion of Lehigh coal, is more economical than an open fire-place, that, therefore, one ton of the coal possesses greater intrinsic value than one cord of shell-bark hickory wood, as it would be equally relevant, to say, that the coal is intrinsically of less value, because the wood may be consumed in a sheet iron stove, which is a much more economical apparatus than the grate.

We will resume the subject by comparing one ton of Lehigh coal, at seven dollars, with one hundred bushels of Newcastle coal, at thirty-five dollars, which are the present prices in this market. As 99 : 700 :: 198 : 1400, from which, it appears that fifty bushels of this coal are precisely equal in value to one ton of Lehigh coal, but as the Newcastle coal will cost seventeen dollars and fifty cents, and the Lehigh coal costs only seven dollars, the latter is the cheaper article of fuel by 150 per cent.

If the value of a chaldron or bushel of the bituminous coal is required, the manner of obtaining a solution of either question, is obvious.

It will be apparent, that although shell-bark hickory has been taken, for convenience, as the standard, to construct the column of comparative values, the economist should take the cheapest article of fuel in the market, as his standard of comparison.

The experiments on the Lehigh, Schuylkill, Susquehanna, and Lackawaxen coals were repeated a number of times in different quantities, but the results were found to be uniformly the same. Considerable difference was found in the results of pine charcoal, when taken promiscuously from different parcels as brought to market, in consequence of the imperfect manner

in which the charring process had been conducted, but as these coals are sold by measure, and not by weight, and as the bulk is not materially diminished in perfecting the process, the loss sustained from this circumstance being in part compensated by the heat disengaged in expelling the remaining inflammable matter, we may consider this defect, in ordinary cases, as unimportant; the result, however, is given for perfect charcoal.

The coak used to experiment upon was produced in the large way, and that which was most free from earthy, or other foreign matter, as well as most perfect in other respects, was selected. The result is less than was anticipated, and shows that the commonly received opinion that it contains as much carbonaceous matter as charcoal, in equal weights, is erroneous, and what is still more erroneous is, the opinion that any given quantity of coak, by *measure*, will in its combustion disengage as much heat as an equal quantity of the coal from which it is produced. One bushel of bituminous coal produces in retorts about one and a half bushels of coak, in consequence of swelling during the process, and yet its specific gravity is stated, in some tables, as nearly equal to the coal.

The composition balls of Lehigh coal, charcoal, and fire clay, were made for the purpose of ascertaining whether a very economical fuel might not be formed of the culm or fine portions of the two former, by combining them with the latter article, as they possess very little value, and the same practice having been adopted with considerable advantage in various parts of Europe.

The fire produced by these balls was found to be very clean and beautiful in its appearance; its superior cleanliness is in consequence of the ashes being retained by the clay, and the balls were found to retain their original shape, after they were deprived of the combustible materials. The beauty of the fire is enhanced by the shape and equality in the size of the balls, which, during the combustion, present uniform luminous faces. No difficulty was found in igniting or perfectly consuming the combustible materials of the balls, and the loss in heat, when compared with the combustion of the same quantity of each

article, in their usual states of aggregation, was found to be only three per cent.

It is proper to state that the experiments were made with the best quality of every article that could be procured, and as some slight difference may exist between wood of different ages, the medium sizes were selected. Those woods and coals which are peculiar to the New England States, were obtained from thence. The Rhode-Island and Worcester coals were procured for me by an obliging friend in Boston, who stated that the coals were selected with care, but, that the Worcester coal being a recent discovery, and the parcel sent having been taken from the surface of the bed, could not be considered as a fair sample of the coal which may be supposed to exist in lower strata.

Many and insuperable difficulties presented themselves, in attempting to ascertain by common methods the weight of dry wood in a cord of each kind. The plan adopted, and which appeared most likely to produce satisfactory results, was as follows. From a pile of swamp white oak of medium size, which had been cut the preceding winter, and weather seasoned during the interval, (this being the state in which the largest portion of wood is sold,) a half cord, or sixty-four cubic feet, was accurately measured, and its weight was found to be 1928 avoirdupois pounds. From this half cord was taken in various sizes, a sufficient number of sticks to allow one piece to be sawn from each, twelve inches long, to produce $\frac{1}{64}$ part of the whole weight, which being done, the pieces of wood were placed in a foot "*corder*," or space twelve inches square, made by nailing four pieces of board together at the ends; but the wood not being found to fill it equally in the first instance, other pieces were substituted, of *equal weight*, until the interstices between the sticks presented a similar appearance to that of wood, as ordinarily piled up for sale.

This parcel of wood was then perfectly dried in an oven, and its solid content ascertained by the quantity of water which it displaced. To perform this operation, a tin box was used, fifteen inches deep, and six inches wide at the open top,

which was set into a large tin funnel, and the water displaced by the wood was conveyed by the latter into an earthen vessel placed underneath for its reception. The pieces of wood were taken separately, and into one end of each, a small awl was inserted a sufficient distance to sustain the weight of the stick, and by which it could be accurately and expeditiously immersed in the water. As the surface of the wood could not be made impervious to water without a change in its bulk, it became necessary to perform the operation with as much dexterity as possible; the wood, however, being perfectly dry, its surface was covered with dust, which caused it to *repel* the water in the first instance, and I found it could be immersed steadily, and yet with such facility, as to be left nearly dry if shaken immediately on being withdrawn from the water, and this was determined by the very slight addition which was found to have been made to its weight by the immersion. For this addition to the weight of the wood, the water used being at 55° Fahrenheit, a correction was made and added to the quantity of water displaced, although a partial compensation may be considered to have taken place by the expansion produced in the wood in consequence of the absorption of this portion of the water.

The water displaced was measured in a deep narrow vessel, provided with a sliding scale, fitted to its interior, for the purpose, and found to be 965 cubic inches, from which the quantity of *plenum*, or solid dry wood, in a cord taken under the circumstances described, was found to be $71\frac{1}{2}$ cubic feet, leaving a deficiency for the interstices and diminution in volume by drying of $56\frac{1}{2}$ cubic feet. Thus,

$$1:965::128:123520, \text{ which } \div 1728 = 71.\tfrac{832}{1728} \text{ cubic feet.}$$

The method taken, it is supposed, will give the average quantity of *combustible matter*, in a cord of wood, as usually sold, it being impossible for me to give a scale adapted to every change in volume produced by the different degrees of humidity, of which the woods are susceptible.

The solid content of a cord of wood being known, if the

specific gravity of any wood is correctly ascertained, its absolute weight may be determined thereby.

The usual method of ascertaining the specific gravity of wood, as laid down in the books, is manifestly incorrect, as the absorption of water, during its immersion, produces an enlargement in the *magnitude* of the body, not compensated for by adding to the water weight, if the body is lighter, (or deducting, if heavier than water,) the weight of water found to have been absorbed, and this absorption must constitute complete saturation before the water weight can be accurately ascertained, because, during this process of absorption, the air being constantly expelled from the body, part of it adheres to it in small globules, and renders it more buoyant, in proportion as this bulk of air is lighter than the same bulk of the body; consequently, the body weighs less than it should do, and this cause of error cannot be counteracted by an attempt to weigh the body "*expeditiously*," as is recommended. During this necessary process of saturating the body with water, the wood increases in magnitude, and its specific gravity will be found less than it should be; and the difference will be seen to be very considerable, when it is stated that the specific gravity of a piece of dry wood, weighing in air 11.15 grains, was, by the common method found to be .556, and the same piece of wood being then dried with great care to its former weight, its specific gravity found by a process free from this objection, (hereafter to be described,) was .619, the difference in which would be 282 lbs. in one cord of wood.

The specific gravity of those bodies which do not change in their *magnitude* by the absorption of water, and which have no *fissures*, may be correctly obtained by the common method, as the water absorbed is *retained* in the body, and can thereby be ascertained, as it will be of the exact weight by which the water weight had been increased or diminished in consequence of the expulsion of an equal bulk of air from the body.

Our object in ascertaining the specific gravities of bodies, is to find the proportion of their weights under the same volume. Now, by the volume of a body, is to be understood

the entire space enclosed within its exterior surface, including its pores and fissures. It is necessary, therefore, in determining the sp. gr. by the usual method of the hydrostatic balance, to use some means for preventing the water from insinuating itself into the pores and fissures of such bodies as are not of a perfectly compact texture. If the article employed for this purpose be of a sp. gr. different from water, and if (as will almost always be the case) it protrude beyond the surface of the body so as to enlarge the bulk, it will be necessary not only to know its weight in air, but its specific gravity; and even then it is difficult to make a satisfactory correction of the water weight in consequence of the change which the article made use of may sustain in its specific gravity by pressure in applying it to the body, and also, from the different specific gravity of different parts of articles not expressly prepared for the purpose.

As it was necessary for me to determine with great accuracy the specific gravities of dry wood, charcoal, and the mineral coals, all of which absorb water and present more or less fissures, and as I wished to relieve myself from liability to inaccuracies from the sources which have been detailed, I determined to make a compound which should be convenient to use, and whose specific gravity should be precisely that of water at 60° Fahrenheit.

This was effected with white wax and yellow rosin; the specific gravity of the former was .967, and of the latter 1.079. The compound was of the best possible consistence, and whether compressed by mechanical means at a low temperature, or expanded by the temperature of water at 120°, it would in either case be unity when brought to the temperature of 60°, and the whole mass was perfectly uniform.

The difficulty of producing this compound was much greater than had been anticipated, and will be apparent, when it is stated that the mass weighed at the commencement about two ounces, taken by arithmetical calculation in the proportions supposed to be necessary, which were 46 grains of rosin to 100 grains of wax, and although the smallest additions supposed

necessary, were made at each time to this mass, from two other masses of the same articles compounded, whose specific gravities were known to be about .995 and 1.005, the mass weighed when finished more than thirty ounces, and required seven days to accomplish the undertaking, and the proportions of the ingredients found to have been used, were about 22 grains of rosin to 100 grains of wax. Having had occasion to use some of this compound within a short time, I regret to say, that the lapse of two years since it was made, has produced a change in its specific gravity, it being now 1.004 in water at the temperature of 60° Fahrenheit.

The pieces of wood being made positively dry, in the manner described for drying those experimented upon, they were covered with the compound described without regard to its weight, and their specific gravities being ascertained, the absolute weight of dry wood in a cord of each was found in the following manner, and will be seen in the table.

The weight of a cubic foot of any substance, whose specific gravity is 1, is known to be very nearly 1000 ounces, or $62\frac{1}{2}$ pounds avoirdupois. Hence, to find the weight of a cord of wood, or $71\frac{1}{2}$ cubic feet of *plenum*, of specific gravity 1, (for example, shell-bark hickory) we have only to multiply 71.5 by 62.5, which gives us 4468.75. Now, to find the weight of a cord of wood, of any other specific gravity, we say, As *unity* is to (4468.75) the weight of a cord at specific gravity 1, so is the given specific gravity, to the weight of a cord at that specific gravity. Thus, for white ash; 1 : 4468.75 : : .772 : 3449.87 pounds. In fact, we have, in any case, merely to multiply 4468.75 by the specific gravity of any other wood, to obtain the weight of a cord of this wood, in pounds and decimals avoirdupois.

The quantity of charcoal which can, by the best conducted process, be obtained from the different woods, was deemed an inquiry of considerable importance, there being great discrepancies in the results of different experimenters on this subject, and from the vast importance and consumption of this article in the arts generally, and particularly in the process of smelting iron ore. For this purpose all attempts hitherto made

in this country to substitute anthracite coals, have proved nugatory; and, as equally unsuccessful results have attended the numerous and well conducted experiments, which have been made in England, Ireland, and Wales, to substitute anthracite coals for coak, in the same important process, it becomes a matter of national interest, that our forests, intended for this purpose, should not be unnecessarily wasted by conducting the charring process in an improper manner, and this can only be ascertained by first knowing the positive quantity of carbon contained in the different woods, from which we shall be able to determine whether any improvements can be made in the process.

Various methods have been adopted by different experimenters on this subject; that most generally used appears to have been charring the woods in dry sand; but I found this objectionable, as the finer portions of the sand were liable to enter the interior of the coal, if it had any fissures, and the weight of the product was too large, while on the other hand, the interstices between the particles of sand were found to admit sufficient air to consume part of the coal, and the product in consequence of this combustion was liable to be found too small. To obviate both these objections, pulverized charcoal, known to have been perfectly charred and dry, was substituted for sand, having ascertained that it could be almost entirely shaken out of the fissures in the coal, and that, should any remain, the error would be immaterial. The pieces of wood were closely packed in it, and presented an inch in thickness of powdered coal between the sides and bottom of the crucible and the wood, and about three inches of powdered coal on the top of the wood, the whole being covered by an inverted crucible luted down. In this latter crucible a small orifice only being made, any air, therefore, which should enter through the pores of the crucible or the aperture at top, would be decomposed before it could reach the wood in the interior, and the air which may be supposed to have existed between the interstices of the powdered coal, or in the coal itself in the first instance, would also be decomposed and rendered inert,

before the wood could be charred. The whole of the woods, which had been previously filed into oblong solids, presenting sharp edges, to detect any loss by fracture, each being designated by notation letters, made by incision, were thus surrounded and exposed in the first instance to a moderate heat in an air furnace, which was increased to a white heat, and so remained for about two hours, during which time additional quantities of powdered coal were introduced through the aperture at the top of the inverted crucible.

The product of charcoal from the several woods obtained in this manner, will be found in the table.

Among the many experiments made to discover the best manner of ascertaining the weight of charcoal product from the different woods, and to satisfy myself whether any loss could take place in a solid piece of coal surrounded by powdered charcoal, a piece of box wood coal without fissures was taken, weighing 23.7 grains, and after having been exposed to a white heat for three hours, was found to weigh 23.1 grains; the loss of $\frac{6}{10}$ of a grain, was, however, undoubtedly, produced by the air contained in the piece of coal, or conjointly with that in the interstices between the powdered coal, contiguous to the piece when first ignited.

A similar experiment was made in clean dry white sand, upon a piece of maple coal without fissures, which had been previously exposed in powdered charcoal to a white heat, and known to be perfectly charred and dry. This piece of coal weighed 26 grains, and lost by the process 6 grains; the surface was found entirely changed from its original hard texture, having become soft, and the colour was changed from slate to jet black, which is often found to be the case in charcoal obtained in the large way, and is always objectionable, as it produces loss both to the collier and consumer.

The charcoal produced by surrounding the wood with powdered coal was found of a slate colour on its surface, dense, sonorous, brittle and equal in all respects to that made in cylinders or retorts for gunpowder, which is known to be much superior to that produced by the ordinary method, even for

common purposes, from its greater durability, although, for these purposes, no particular necessity exists that the pyroligneous acid and tar should be perfectly expelled. From the preceding experiment in sand, it occurred to me that an important improvement might be made in the common process, by filling the interstices between the sticks of wood with the culm or fine coal left on the ground after the large coal has been drawn from the pit, and by covering the wood more perfectly than is usually done. In this way we may more perfectly prevent the access of the air, which is not only destructive in many cases to a large portion of the coal, but also renders what remains, less valuable.

That my remarks on this subject may not be considered entirely theoretical, it is proper to state, that an intelligent collier in New Jersey applied in a partial manner the plan proposed, and found the product to be about 10 per cent. more in quantity by measure, than he had ever before obtained from the same kind and quantity of wood, and I found the coal when brought to market nearly 20 per cent. heavier than usual, and as an evidence that the coal had been well charred, a circumstance which is too often neglected, the hydrogen gas appeared to have been almost entirely expelled, and it lost very little in weight by exposing it to a red heat in powdered charcoal.

The quality of this coal was considered by competent judges to be superior to any other ever offered in this market, and was as cleanly to handle as the anthracite coals, and sold readily at an advanced price.

From an examination made during the last summer, of the common manner of piling and covering wood which is to be converted into charcoal, the practice of piling it two and three tiers in height, appears to be objectionable for two reasons; the first is, that the second and third tiers cannot be so well defended from the air as the first, which rests upon the ground, this being a better barrier against the air, than the former can be made to present; and the second is, that this disposition of the wood is not favourable for producing the ignition of the whole mass at one and the same time, the usual practice being either to commence the ignition in the centre of the upper tier,

or, in other cases, to drop the fire into a hole, or chimney, left in the centre of the pile which extends to the bottom, or ground; and by giving air holes at the sides of the pit, to use the language of the colliers, the fire is said to be "drawn to the sides of the pit."

It is very true, that the fire does eventually extend to the sides of the pit; but a much more uniform and speedy process, and by which less loss would be sustained, would be to place the fire in the first instance in a number of holes at the sides, near the bottom, leaving an opening at the top by which the heat generated at the sides would be communicated to the wood in the interior, and facilitate the uniform ignition of the whole mass, and the moment this is effected, let the holes at the sides be closed, and that at the top may be lessened, but should not be wholly closed, until the extrication of hydrogen gas has nearly ceased, which, from its prodigious expansion, sometimes bursts the pits, and as this generally occurs when the wood is well covered, and sometimes produces very injurious effects, by firing the adjacent woods, (as the column of flame has been known to extend from twenty to thirty feet,) it has probably led many colliers into the belief that the proper remedy is to give the wood a slight covering, by which numerous escapes are allowed for the gas; but in effecting this object, as the holes at the sides are left open, a very strong current is produced through the pit by the slight covering, and another evil is produced, that of burning through the sides of the pit.

In those instances where pits have been known to burst, when well covered, the cause may probably be traced to having closed the chimney at the top too soon, this being generally done in about fifteen minutes, and having left those open at the sides too long, as the gas will make its escape in some manner, which should be provided for, and this provision is as necessary to a coal pit, as the safety valve is to a steam boiler.

Both the objections which have been alluded to against piling the wood two or three tiers high, may in part be remedied by changing the manner of igniting the wood as proposed, and if

clay can be procured, (with sand on the top, to fill the cracks as it dries,) as a covering, which should be preferred in all cases, the evils may be reduced; but the best manner is, undoubtedly, to pile the wood in single lengths, and if the fine coal is used to fill the interstices, and can be made subservient in its combustion to produce the required heat or any portion of the heat necessary to char the wood, that portion which can be so used is as effectual as the combustion of an equal portion of the char. The process being, when conducted in retorts, similar to that of distillation, the qualities of the wood necessary to be expelled being volatile, no necessity exists that any combustion should take place either in the wood or char; yet this cannot be entirely prevented, in the common process, unless some means are devised to burn the hydrogen gas which escapes, and make it applicable to produce the heat necessary to char the wood as is done when the process is conducted in retorts. The hard texture of the coal will be in proportion to the heat given it, and the exclusion of air; the advantage therefore of using clay will be obvious from its being a bad conductor of heat, and a good barrier to exclude the air.

I have been informed by a gentleman well acquainted with the iron works in this state, that in consequence of the slow growth of the extensive forests belonging to the same, not being sufficient to furnish a constant supply of charcoal, many of the works are obliged to suspend their operations, about three months in each year, by which very great loss is sustained. If an improvement can be made in the manner of producing the charcoal required, by which these works, and all others similarly situated shall be enabled, from their present forests, to continue their operations without interruption, such an improvement must be considered as important, not only to individuals, but to the community generally.

A series of experiments was made on a large number of woods, to determine the difference, if any existed, in the product of charcoal from green and dry wood; and these being taken from the same sticks in equal weights when green, they would both contain the same quantity of ligneous matter. The

product was not found to be essentially different, but, invariably, rather larger from the dry than from the green wood, and the specific gravity of the former was also greater; I have no hesitation, however, in saying that there will be less loss in charring wood in the large way by using dry wood, as it can be ignited more equally, and with greater facility.

It is my intention, so soon as my other avocations will permit, to make some experiments in the charring process in the large way, and to use the fine coal as suggested, for which purpose a number of cords of wood have been cut for a considerable period of time.

Dead wood was found to produce the same quantity of charcoal as the same wood in a living state, and the limbs of trees produced coal of much greater density than the trunk. Among the most dense woods, stove dry ebony, sp. gr. 1.090, gave a product of charcoal from 100 parts of wood, of 33.82, which is larger than was obtained from any other wood, and its specific gravity was also greater, being .888; its fracture so much resembles that of some of the mineral coals, that it is difficult to say in what respects they differ. Stove dry live oak, sp. gr. .942, gave 32.43, sp. gr. .591. Tortoise-shell wood, sp. gr. 1.212, gave 30.31, sp. gr. .866. Cocoa, sp. gr. 1.231, gave 28.53, sp. gr. .742. Turkey box, sp. gr. .933, gave 27.24, sp. gr. .622.

A piece of box wood polished, lost very little of its lustre by charring in powdered coal, and the beautiful variations in the grain of the wood were as apparent in the coal as in the wood, and this experiment may be considered as conclusive, as to the complete exclusion of air by this process.

It does not appear from the products of charcoal from the different woods, that their density or durability is to be attributed to the quantity of carbon they contain. As the woods differ materially in the quantity of charcoal product by measure, it appeared necessary to give the product from a cord of each in bushels, and as the value of these can only be determined by their weight, this also appeared necessary, both of which will be found in the table.

The bushel generally used in this country contains 2150.4 cubic inches, but as coals are sold by what is termed "*rounded measure*," or partially heaped, it became necessary to ascertain the cubical content of a body of coal thus measured. For this purpose one bushel of charcoal was made perfectly dry, and the mean specific gravity of a large number of pieces was found to be .285, and the weight of the bushel of coal was fifteen pounds avoirdupois, or 105000 grains, and the absolute weight of a cubic foot of coal whose specific gravity is .285, is 124687 grains, and a cubic foot being 1728 cubic inches, then we have the following statement: As 124687 : 1728 : : 105000 : 1455 solid inches of coal in the bushel, which being known, the absolute weight of a bushel of each of the coals was calculated from their specific gravities, in the following manner:

The weight of a cubic foot, or 1728 cubic inches of any substance, whose specific gravity is 1, being 1000 ounces, consequently the weight in ounces of a bushel containing 1455 cubic inches of any substance, of the same specific gravity, will be found by the following proportion:

As 1728 : 1000 : : 1455 : 842 = 52.62 pounds.

Now to find the weight of a bushel of a substance of any other specific gravity, we say; As *unity* is to (52.62) the weight of a bushel at specific gravity 1, so is the given specific gravity, to the weight of a bushel at that specific gravity. Thus for white ash charcoal, we have, As 1 : 52.62 : : .547 : 28.78 pounds.

From a number of comparisons, made by actual measurement, of different mineral coals, it is believed the weights expressed in the table will be found sufficiently correct in every instance.

The hydrostatic balance made use of to ascertain the specific gravities of the different bodies expressed in the table, is sensibly affected by $\frac{1}{100}$ part of a grain, when not loaded, and the weights were made to twentieth parts of a grain in every instance.

From experiments made to ascertain the weight of moisture absorbed by the different woods, which had previously been

made perfectly dry, and afterwards exposed in a room in which no fire was made during a period of twelve months, the average absorption by weight, for this period, was found to be 10 per cent. in forty six different woods, and 8 per cent. in the driest states of the atmosphere, and an unexpected coincidence was found to exist in the absorption by weight of forty six pieces of charcoal made from the same kinds of wood, and similarly exposed, the latter being also 8 per cent.

The quantity of moisture absorbed by the woods individually, was not found to diminish with their increase in density; while it was found that the green woods, in drying, uniformly lost less in weight in proportion to their greater density. Hickory wood taken green, and made absolutely dry, experienced a diminution in its weight of 37½ per cent., white oak, 41 per cent. and soft maple, 48 per cent.; a cord of the latter will therefore weigh nearly twice as much when green as when dry.

If we assume the mean quantity of moisture in the woods, when green, as 42 per cent., the great disadvantage of attempting to burn wood in this state must be obvious, as in every 100 pounds of this compound of wood and water, 42 pounds of aqueous matter must be expelled from the wood, and as the capacity of water for absorbing heat is nearly as 4 to 1, when compared with air, and probably greater during its conversion into vapour, which must be effected before it can escape, the loss of heat must consequently be very great.

The necessity of speaking thus theoretically on this point, is regretted; but, it will be apparent, that this question of loss cannot be solved by my apparatus, as the vapour would be condensed in the pipe of the stove, and the heat would thereby be imparted to the room, which, under ordinary circumstances, escapes into the chimney.

The average weight of moisture in different woods which have been weather seasoned from eight to twelve months, will not be found to vary materially from 25 per cent. of their weight; every economist, therefore, will see the propriety of keeping his wood under cover in all cases where this is practicable.

Numerous experiments have been made to determine the law which obtains in the cooling of heated bodies. Although my apparatus did not admit of making experiments on this subject at high temperatures, yet it appears in one respect better adapted for the purpose than any other which has, to my knowledge, been made use of, as we are enabled to maintain both the heated body and the refrigerating medium at the same *difference* of temperature, for a sufficient period of time, to determine the question with accuracy. My experiments consisted in maintaining the temperature of the interior room 10°, 20°, 30°, and 40° above the temperature of the exterior room for the same period of time, and the quantity of fuel required was found to be directly proportional to the increased difference in temperature. These results are in agreement with the assumption of Newton, the geometrical law of Richmann, and also correspond at these differences of temperature with the experiments of MM. Dulong and Petit, although the latter gentlemen found very different results at higher temperatures.

The usual method which has been adopted to determine this question, by finding the period which fluids require, when heated, to cool through a given number of degrees in different parts of the scale of a thermometer, appears liable to some objections, which it becomes me, however, to notice with deference. The shape or size of the containing vessel is not, perhaps, material, but as spheres have been most generally used, my remarks will be confined to that shape.

We will, for illustration, assume the containing vessel to be the bulb of a thermometer two inches in diameter, and filled with mercury. This we will suppose to be heated to 300° of Fahrenheit, and placed *in vacuo*, in which case it is said to lose its heat by radiation only. Now, as the stratum of mercury in contact with the bulb, parts with its heat, it contracts and occupies less space in the bulb, which causes a portion of that within the tube to sink into the bulb in order to supply the deficiency. This exterior stratum must then be supposed, from its loss of heat, to have acquired greater density, and to leave the sides of

the bulb; hence, *motion* in the fluid commences, and in proportion to its heat will be its fluidity, and consequently, the velocity with which the change will be made, and as the strata lessen in volume as they approach the centre of the bulb, their heat must either be transmitted through the exterior intervening strata, or be subject to the necessary delay in coming in contact with the bulb, in consequence of the decreasing velocity with which the changes are made; and, in either case, the cooling process will be retarded. If we suppose the fluid, under the circumstances described, incapable of locomotion, it will not be denied that the interior strata will require more time to impart the same heat than the exterior, consequently, proportional to the cooling of the body must be the increased time required to deprive it of any given number of degrees.

Experiments upon this subject would be much more satisfactory, and would probably give different results from those hitherto obtained at *high* temperatures, by using an apparatus which should admit of maintaining the heat at fixed points upon the scale of the thermometer; in which case motion in the fluid would be immaterial, and an equally heated surface would always be exposed to the refrigerating medium.

Experiments to determine the comparative loss of Heat sustained by different constructions of apparatus ordinarily used for the combustion of Fuel.

The comparative loss of heat which arises from the different manner in which fuel is consumed, is a subject intimately connected with the question of economy in its use, but it is a distinct subject of inquiry from the former investigation, which was to determine the comparative heat disengaged in the combustion of the various kinds of fuel. It is presumed the remarks which have already been made, in anticipation, on this point, in detailing the first course of experiments, (at page 23.) will be considered conclusive.

For the purpose of performing these experiments, a slight alteration, only, of the interior room, was required.

The chimney of the exterior room being situate within twelve inches of the board partition on the east side of the interior room, an opening was made through the partition of a sufficient size fairly to expose the fire-place of the chimney to the interior room; the sides, top, and bottom of this aperture were then closed by boards perfectly tight, and may now be considered as forming part of the interior room.

All the apparatus, with the exception of the stove, remained the same, and was made use of as has been before described.

Those constructions of apparatus in most common use, and of proper size for the room were selected. The experiments could not, without great inconvenience, be extended so as to embrace all the inventions which have been presented to the public as improvements upon these constructions, but it is believed those selected will be sufficient for the object of the inquiry.

This course of experiments was conducted on the same plan as the former, namely, by determining the period of *time* which the air of the interior room could be maintained 10° of temperature above that of the exterior room, in the combustion of equal quantities of fuel, by weight, in each apparatus. In some cases, indeed, it was necessary to use larger quantities of fuel than in others, in order to make satisfactory experiments, yet the results are given for equal weights, and exhibit the time which the air of the room was thus maintained by each apparatus, and are compared with the time which the same weight and kind of fuel had maintained the same difference of temperature in using apparatus No. 9, in the former state of the room; a correction having been made for the slight increase in its size, in consequence of the alteration which has been described. The fuel used in all the experiments was shell-bark hickory wood, of the same quality, and absolutely dry.

It had been apprehended that considerable difficulty would be experienced in producing the required equality in the tem-

perature of the interior room, from the absence of proper means, in some of the apparatus experimented upon, to regulate the combustion; but from very few trials with each, it was found less difficult than had been anticipated, and that the difficulty could be entirely avoided by making the quantity of fuel administered to the fire, the regulator of the rapidity and extent of combustion necessary to be produced, which was effected by using the wood in small pieces.

The results have been thrown into a tabular form, and exhibit, as before stated, the comparison of each apparatus with No. 9, in which it is assumed that no loss of heat is sustained, this assumption being necessary, for the purpose of determining the comparative *loss* of heat sustained by each apparatus, which is the object of the experiments.

The manner of obtaining the results in *time*, having been stated, it is evident, that, as the same quantity of fuel was consumed in every experiment, consequently the same quantity of heat must have been generated. In all the experiments, (except the standard experiment No. 9,) we find part of the heat escaped by the pipe or flue of the grate and fire-place into the chimney, and was lost, and proportional to this loss must have been the quantity of the fuel required to be consumed in a given time, to maintain the temperature of the room, and, consequently, the duration of each experiment was proportionally affected thereby. The numbers, therefore, which express the duration of each experiment, are proportional to the heat *saved*, and assuming the positive quantity of heat generated as 100, this being the result of apparatus No. 9, if the time occupied in any other experiment is deducted from 100, the remainder gives the positive *loss* sustained in every hundred parts of heat generated by using this apparatus, and by which we determine that in using No. 1, as only 10 parts in every hundred parts of heat generated are saved, consequently we lose 90 per cent of heat.

As the important difference which exists in the quantity of fuel required to be consumed in different apparatus to produce the same effect, might not in all instances be obvious by a cur-

sory inspection of the numbers in the first column of the table, the second column of numbers has been inserted to facilitate these comparisons, and the great disparity in the quantity of fuel required to produce the same effect in No. 1 and No. 2, may, at first view, appear paradoxical, if compared with the quantity of heat saved by each, from 100 parts generated, as only 8 parts more heat are saved by No. 2, than is saved by No. 1, and yet the positive saving in fuel by using No. 2, is nearly 45 per cent.

To find the numbers in the second column, we assume the fuel used in all the experiments as 100; and for the facility of comparison, we will say this quantity of fuel maintained the temperature of the room 100 minutes when consumed in apparatus No. 9. In experiment No. 1, we find this quantity of fuel maintained the temperature of the room only 10 minutes, and, consequently, it would have required 10 times as much fuel as apparatus No. 9, (or 1000,) to maintain the room at the same temperature for 100 minutes. In the same manner the other numbers are found.

The proportion for the experiments will be clearly explained in the following manner: As the time of the experiment is to the quantity of fuel consumed, so is the assumed time of comparison, to the fuel that would be required for that time. Thus for experiment No. 2: As 18 : 100 : : 100 : 555.

By an examination of the numbers in the second column of the table, it will be seen that one dollar expended in fuel consumed in apparatus No. 9, is as effective as ten dollars expended in the same kind of fuel consumed in No. 1, the same quantity of heat being imparted to the room in both cases. The comparison may be extended in the same manner between any two experiments inserted in the table, and the figures in the second column will be found to express the relative value of fuel for each apparatus, in dollars and cents, by adding a decimal point at the left hand of the two last figures.

Experiments No. 6, 7, and 8, were made with the same stove for the purpose of determining the difference in the loss of heat by different constructions and positions of pipe of the

same length, which in all other respects were similar. From these experiments it will be seen, that the same length of pipe in elbow joints is much more efficacious in imparting heat to the room than straight pipe, and as the length of pipe producing a *descending* current, was nearly equal in experiments No. 6 and No. 8, the great advantage which has been supposed to be derived from the descending current, does not appear to exist, although it is undoubtedly more efficacious than the same length and position of pipe producing an *ascending* current, as the velocity of the current in the former is diminished by the increased resistance which must necessarily be overcome in its descent, while the latter gives greater facility for the heated air to escape than is given by any other position in which the pipe can be placed. Experiment No. 7 shows that pipe placed horizontally is more efficacious in imparting heat, than when placed in a vertical position either for an ascending or descending current.

The causes which operate to render the same length of pipe in elbow joints more efficacious than any other construction, may be satisfactorily explained. The shape of the pipe forces the current of heated air to make abrupt turns, in doing which it impinges against the elbows with sufficient force to invert its internal arrangement, by which change from its former relative situation with the sides of the pipe, a new stratum of hot air from the interior of the current, is brought more frequently in contact with the sides of the pipe, which facilitates the process of imparting heat, particularly by being brought in contact with the lower half of the horizontal part of the pipe, which is necessarily the coldest from various causes, and is of very little service in imparting heat to the room without the aid of elbow joints.

From experiment No. 8, an important inference may be drawn; that the advantage gained under ordinary circumstanes, by increasing the length of the pipe, has a limit very far short of that which is found to be necessary to impart *all* the heat generated to the air of the room, as in this experiment, only five parts of the heat were lost in using $13\frac{1}{2}$ feet of pipe, con-

sisting of nine elbow joints; whereas, in apparatus No. 9, eight additional elbow joints, with sixteen and a half feet of straight pipe, amounting together to $28\frac{1}{2}$ feet of pipe, were required to save these five parts of the heat which would otherwise have escaped into the chimney. The reason for this limitation will appear evident, by reflecting that a heated body loses less in equal periods of time, as its temperature approaches that of the surrounding refrigerating medium, and that the loss of heat will be in the proportion which the volume of air in the pipe bears to the volume of air in the room; and, also, proportional to their difference of temperature.

It must not, however, be inferred from this experiment, that $13\frac{1}{2}$ feet of pipe of any *diameter*, and thickness of iron, made into elbow joints, will produce the same effect; as the length will require to be increased with the increase in its diameter, and this will appear obvious, from the fact, that the surface of the pipe does not increase in the ratio of its area or contents of heated air, and as this surface is the medium by which the heat is imparted to the room, and that being effected principally by contact with the sides of the pipe, greater length will be required to produce this necessary contact.

The great advantage of sheet iron stoves, is obvious, from the slight obstruction which they present to the rapid communication of the heat generated, to the air of the room.

From experiment No. 2, the advantage gained by lessening the current of air into the chimney is clearly demonstrated; this being the principal cause why this apparatus is more efficacious in warming the room than No. 1; and this advantage does not arise so much from the excess of heat which enters the room by using No. 2, as from the diminished quantity of cold air necessary to be admitted to supply the place of the air that *has been heated*, and of which, by using No. 1, the room is constantly deprived in much larger volume than by No. 2. The advantage derived from using stove pipe of small diameter, arises from the same cause, and whether the velocity of the current of heated air is diminished by the construction, position, or length of the pipe, or its volume is diminished by reducing the diameter, the same effect is produced in every case.

I am not in possession of the results of any experiments, if such have ever been made, to determine the ratio of friction experienced by air, when compared with water, in their passage through pipes, under the same pressure. That air does, however, experience a diminution in its velocity from this cause, will not, it is supposed, be doubted, and this must affect, very materially, the current of air through pipes and chimneys.

In practical hydraulics, it is well known, that, without altering the column of pressure, the quantity of water discharged is greatly diminished, by merely lengthening the conduit-pipe. "Comparing the experiments on the flow of water through conduit-pipes, as recited in Bossuet's *Hydrodynamique*, I find, after making the proper reductions, that the velocity of projection from the bottom of a cistern, is diminished about five times in the passage through an horizontal tube of one inch in diameter, and fifteen feet long. Consequently, while one part of the actuating force is discharged from the orifice, twenty-four parts are consumed in gliding against the sides of the pipe. Every particle contained must hence have repeated its contact no less than twenty-four times, before it made its escape; that is, the whole column of fluid must have inverted its internal arrangement at each interval of 7½ inches."*

The principal article of fuel used in the United States, is forest wood, which, from necessity, or choice, will continue to be so in many sections of the country, notwithstanding the abundant supply of anthracite and bituminous coals already discovered in some of the States.

The difficulty of consuming small quantities of anthracite coal in open grates, must operate to prevent its general introduction into use, unless this difficulty can be removed; any suggestions, therefore, which may possibly tend to lessen this objection to an article of such vast importance to the community, will not be considered irrelevant to my subject.

It is very well known, that no particular difficulty is experienced, under ordinary circumstances, in consuming small

* Mr. Leslie on Heat, 308.

quantities of this coal in sheet iron cylinder stoves lined with fire brick; and it is as well known, that an equally small quantity of this coal cannot be consumed in an open grate. The inference, therefore, which should be drawn from the knowledge of these facts, is, that the open grate is an improperly constructed apparatus to obtain the desired object, independent of the deleterious gas which it imparts to the room. The question which then presents itself, is, what are the qualities possessed by the former apparatus in which the latter is deficient?

In the former, the coal is known to be completely surrounded by a thick substance, which, when heated, retains it with great tenacity. The air admitted is in small quantity, and, from the construction of the stove, it is necessarily considerably elevated in its temperature, before it comes in contact with the burning body. We infer from these facts, that anthracite coal requires a very high temperature to produce ignition, and, as we know that combustion cannot take place without this prerequisite, the necessary means to effect it, are, consequently, indispensable. We also infer, that the commonly received opinion, that this coal requires a very large quantity of air, or "strong draught," to carry on its combustion, is not correct; the converse of this opinion being nearer true; and this may in part be demonstrated by an examination of a single piece of this coal which has been ignited. If we break the piece of coal, the interior will present its original black colour and lustre, with the exception of an inconsiderable portion near the surface; the body of the coal being sufficiently dense to exclude the access of air, no combustion of its interior can take place, and, consequently, the quantity of air necessary to be admitted to the coals, is nearly proportional to the quantity of coal contained in *their surfaces*, but not in proportion to their positive quantity, as would be nearer the case, if this article were as pervious to air as charcoal. Any excess of air, therefore, is injurious in proportion as the quantity exceeds that which can unite with what is termed the combustible or base, inasmuch as it tends to reduce its temperature, and thereby renders it

less capable of rapid union with the air, to produce the combustion; and as each successive portion of air in excess robs the combustible of its heat, we see the fire languish for a short period, and then expire.

Although atmospheric air is generally necessary to support combustion, an excess of it, it is well known, will, in some cases, extinguish a burning body, as expeditiously as water; and from this circumstance it may be inferred that, for ignition. the air requires to be heated as well as the combustible body. We may also infer, that the intensity of heat produced by the union of the two bodies will be proportional to the excess with which their united heats exceed their mean heat of ignition.

Having had occasion, during the past winter, to warm two warehouses, of different sizes, and it being necessary that the temperature should be permanent during the night season, two cylinder sheet iron stoves, of ordinary construction, lined with fire brick, were procured, of different sizes, which were supplied with Lehigh coal.

The construction of the stoves being favourable to apply on a large scale what I had found so advantageous in my experiment stove, there being considerable space between the grate and the bottom of the ash pan, this space was converted into a reservoir for heating the air, by closing the apertures usually made for its admission in the front of the ash pan. During the igniting process, the ash pan was drawn out, but when this was effected, it was closed as perfectly as its construction would admit, leaving only the small crevices at its junction with the body of the stove for the admission of air, and although the largest stove usually contained more than half a bushel of coal, this supply of air was found ample for producing intense combustion, and the quantity of coal remaining on the grate unconsumed, was found to be much less than when the stove was supplied with a larger quantity of air, and a very important saving was made in the heat by the diminished quantity and velocity with which the current of heated air passed into the chimney. Very important improvements may be made in the construction of sheet iron stoves, for burning anthracite coal.

and if provision is made for supplying the burning body with *intensely heated air*, any required quantity of coal may be consumed, and the present manner of lining them with thick brick may be entirely dispensed with, by substituting either thin tiles, or a thin coating of clay lute, sufficient to preserve the iron from fusion or oxidation, and as this would present less obstruction to the speedy communication of the heat generated to the air of the room, consequently less would escape into the chimney.

In examining the construction of the open parlour grate, we do not find in it one entire quality possessed by the close stove; the only one which bears any approach to similarity, is that three sides of the grate are lined with fire brick, but as the fourth is almost wholly exposed, its utility is thereby defeated.

It is admitted that the combustion is very perfect and rapid, when the sheet iron door, or "*blower*," as it is technically termed, is applied to close the front of the grate; and this must be a necessary consequence, as its application transforms the open grate into a powerful *air furnace*, by which the space for the admission of air is very much reduced, and the air is also, probably, reduced in quantity, this not being compensated by its increased velocity, and as the blower defends the body of coal in front from the cold air, to which it was before exposed, the required elevation in temperature is effected and maintained without difficulty.

It is only by radiation that any heat is imparted to the room from coal consumed in open grates, and as the radiated heat is known to be very small from the surface of that portion of coal which is exposed to the front or open part of the grate, the amount of heat imparted to the room would not probably be diminished, but rather increased, by using a thin plate of cast iron for the front of the grate, by which the difficulty of consuming small quantities of coal would be very much diminished; and this would not be less agreeable in its appearance than the equally *sombre* aspect presented by the unignited coal in the front of the generality of small grates, and particularly as the top of the coal would be exposed to view, and present a more luminous appearance.

Although iron is a good conductor of heat, the plate suggested would become sufficiently heated to maintain the necessary temperature of the coal to carry on the combustion of the surface exposed to it, with the exception of the points actually in contact with it, which would be unimportant; and this being the case, its conducting power would, in other respects, be obviously advantageous, and no danger of melting the iron, in this situation, need be apprehended. If, however, danger from melting or oxidation of the iron is feared, as a flat plate of iron could not be permanently covered with any composition of clay, it should be made circular, and defended at the top and bottom by a flange projecting on the inside, the required thickness of the clay. In addition to the plate suggested to cover the front of the grate, a still further improvement might be made by enclosing the ash pit also, both of which might be done with one plate of iron, and the grate for sustaining the coal might rest upon cleats projecting from the interior, taking care to give sufficient room for the expansion of the grate, to prevent its being pressed outwards. A door for the removal of ashes and the admission of air would be required, by which the necessary quantity of air could be admitted without an excess. This construction would also be favourable for heating the air which is to supply the combustible body, the advantage of which must be obvious, when we reflect on the necessity of cooling the burning body as little as possible. By the greater expansion of the air, the quantity which comes in contact with the burning body would be less in excess, at any one time, and better adapted to attain the object; the contact being more frequent, from its increased velocity, the quantity actually united in any given time, would probably be greater, and more heat would consequently be produced. This construction, besides the advantages already stated, would be more cleanly than the open grate, would not require the blower, and could also be made use of for culinary purposes, which is a very desirable object to be attained.

The construction of many grates is very objectionable, in an important particular not yet noticed, which is, making the

receptacle for the coal of greater length than it has breadth or depth, by which the body of coal is not as much heated, and requires to be replenished more frequently to maintain the relative position of the coal, necessary to continue the combustion. A much better shape, and which would require less coal at any one time, would be in the proportions of twelve inches deep, to eight inches square at the top, and gradually diminished to six inches at the bottom, by which the heat generated in the combustion of the coal at the lower part of the grate, in its passage to escape into the chimney, would come in contact with nearly the whole body of coal, and keep it heated, which cannot be the case in the former shape, supposing the contents of the two grates, and the coal in each to be equal; and if we suppose them to be only *half filled* with coal, the position of that in the deep grate, will be very favourable for combustion, although less in quantity; while that in the shallow grate, from the unfavourable situation in which it is placed, would scarcely burn at all. The advantage of placing the body of coal in a deep grate, as described, may be illustrated by the well known fact, that a stick of wood burns much more rapidly in a vertical, than in a horizontal position, and for the reason already described.

Being well aware of the strong predilection in favour of those constructions which will permit the burning body to be seen, which, with other reasons, prevents the use of close stoves in many instances, and particularly where elegance is required, the necessity is apparent, that some new construction should be devised, which can be substituted for the open grate, that will obviate the difficulty, not only of consuming anthracite coal in small quantities, for rooms of small dimensions, but, what is a still greater objection made to its use generally, that the quantity cannot be varied to meet the changes in the temperature of the atmosphere.

In the plan which I will venture to suggest, a partial compromise must be made in the first particular stated, but all the others may be realized.

In those instances where simplicity of construction is requir-

ed, take a cylinder, or rather, an inverted conical frustum, of cast iron, of any required thickness and diameter, and of sufficient height to form the receptacle for the coal and ashes; insert a grate at a sufficient height from the bottom to leave the required room for the ash pit, which should be provided with a door to remove the ashes and unconsumed coal, as usual in close stoves, and, also, to regulate the admission of air, which may be heated as in those stoves. This cylinder may be bricked in the ordinary manner on the outside; and this can be done with greater facility than for the grate, and the cylinder will remain more permanently fixed, as it will rest on the hearth. From the satisfactory experiments which have been made in double cylinder stoves, in which the interior cylinder is made of cast iron, without any coating of clay, it is not probable that this construction would require it. In those instances in which beauty of construction must be consulted, the ornamental parts or appendages to the open grates may be added; the only change suggested, being the substitution of a cylinder, or other shape more desirable, of cast iron, in place of the open grate.

The particular requisites necessary to be attended to in the construction of any apparatus intended for the combustion of anthracite coal, in small quantities, having been sufficiently, and, perhaps, tediously expatiated upon, those whose business it is to construct, will apply any suggestions which may be considered as valuable.

Before closing my paper, I cannot forbear making a few desultory remarks; and, first, on the commonly received opinion, that the "draught" of a chimney, or, more properly, the current of air through it, has greater velocity under one degree of barometric pressure than another, and that this is the cause why a combustible body burns better at one time than another.

That the velocity of the current cannot be greater under one degree of atmospheric pressure than another, *cæteris paribus*, may be satisfactorily shown, by supposing a room, with one window open, in which is a fire-place and chimney, and, that the temperature of the air in the room, and that within the

chimney, is the same as the temperature of the atmosphere. No current of air would be found to pass either up or down the chimney, because the pressure of the column of air in the room would be counterbalanced by the equal pressure of the column of air within the chimney, and, consequently, both must remain stationary. If the temperature of the air within the chimney be elevated by any means, it expands, and becomes specifically lighter, and an ascending current will be produced; and if the same elevation of temperature remain, and we suppose any change, however great, in the pressure of the atmosphere, as that change must, of necessity, operate on both columns of air, consequently, the velocity of the current must remain the same. The current of air through a chimney, being wholly an artificial production, its velocity will always be proportional to its *temperature* above that of the exterior air, whereby the column of air in the chimney being rendered lighter than the exterior column, yields to its superior pressure, and thus the current is established.

If the air in the room is warmer than that in the chimney, a descending current will be produced; which shows the propriety of closing, during the winter season, those fire-places not used, to prevent the descent of cold air and smoke from the adjoining flues; and the advantage of leaving them open during the hot season, when the exterior air is known to be at a lower temperature than the rooms with which they are connected.

The existence of counter currents in a chimney, when in use, and properly supplied with air, spoken of by some writers on this subject, appears to be an illusion, produced by eddies in the air, at the sides of the chimney, as it enters from the room, as it would be difficult to assign any satisfactory cause for such an effect under the circumstances described.

In tight rooms, where fire-places are left open, and are not in use, counter currents will exist, so long as difference in temperature exists between the air of the room and the external atmosphere.

In those instances where the room is too tight to admit air in sufficient quantity to supply the current necessary to take

off the smoke, a descending current is produced, and the smoke is driven into the room as a necessary consequence. The passage of the external air through the small crevices of the room, is not only diminished by the increased friction which it sustains in passing through a large number of crevices, instead of only one of equal capacity, but the pressure is absolutely prevented from exerting its full influence in raising the column of air within the chimney, by which the smoke is made to ascend. If we open a window, the air within the chimney, which before was the heavier column, will become the lighter, and consequently the current will be inverted, and the evil thereby instantly corrected.

It is not my intention to notice the various causes which operate to produce what are termed "bad draughts" to chimneys; there is one cause however of considerable importance, which will be noticed. Chimneys which are new, are found very frequently, if not invariably, to smoke, when an attempt is made to use them before they become perfectly dry. This being attributed to their bad construction in many cases, alterations are consequently made, without knowing the true cause, which will generally be found to be entirely owing to their not being dry. The materials of which they are composed being damp, they are consequently good conductors of heat, and unless very large fires are made, it is difficult to elevate the temperature of the air, throughout the chimney, sufficiently, to produce an ascending current; but when the chimney becomes dry, and covered with carbonaceous matter, it presents a bad conducting surface, and, if then found to smoke, this may be attributed to its bad construction, for which, however, no necessity exists in any case, save that the highly important class of artisans, who wield the trowel, have, too generally, discarded science from their craft.

A sufficient quantity of air must be admitted into every room to supply the demands of respiration and combustion, but any excess is injurious. The usual manner of admitting air for these purposes, through the joints or crevices of the doors, windows, and other parts of the room, appears very objectionable.

as the cold air, thus admitted, is very annoying in its passage to the fire-place, and particularly to those seated near the doors or windows. Now, these inconveniences may be entirely avoided, and all parts of the room rendered equally comfortable, by furnishing the room, as is now done in some instances, with a supply-pipe, near the fire-place, for the admission of air. In this pipe there should be a valve, to regulate the quantity of air necessary to be admitted, by which the pressure of the external air, at the joints, or crevices, may not only be wholly taken off, but an outward current produced, through the crevices at the higher parts of the room.

The objection which has been made to this manner of admitting the air, that it does not change the air in the room sufficiently for respiration, appears to be gratuitous, and has been disproved by experience, in rooms of ordinary size, when not unusually crowded.

An additional improvement, to obviate the inconvenience experienced by over-heated or crowded rooms, would be to furnish a ventilator in the chimney, near the ceiling; but the most rational plan, in these cases, would be to remove the cause, by diminishing the fire.

Having shown very clearly, during the preceding remarks, that the reason why a combustible burns better at one time than another, cannot be owing to any change in the velocity of the current within a chimney, in consequence of changes in the pressure of the atmosphere, it becomes obligatory on me, as an objector to this opinion, to assign a more satisfactory cause.

The fact that combustible bodies generally burn better, when the barometer is at 30, than when it is at 28 inches, other things being equal, is admitted. The principal cause of this, appears to be, that the air is generally drier, and better adapted to produce rapid combustion, having less aqueous vapour mechanically mixed with it. Now moist air retards combustion, and cools the burning body, more than dry air, because it possesses a greater capacity for heat, and, consequently, requires more from the burning body to raise its temperature to

the point of ignition. In chimney fire-places, it is generally observed, that wood fires burn most rapidly in cold weather; and, even while the air of the room is quite cold, they are known to burn very well. This fact will probably be urged, to disprove the necessity of heating the air, to produce more complete combustion in anthracite coal. It should be recollected, however, that wood ignites at a much lower temperature, and, that in very cold weather, a much larger quantity is required to be in combustion at one time, than in moderate weather; and, consequently, that the air within a few feet of the fire, and before it comes in contact with it, is more heated than it is at the same distance in moderate weather, when less fire is required.

The intense heat produced by an air furnace, does not appear to be in consequence of an increase in the volúme of air, as those furnaces which are said to have the strongest "draught," will be found to have the most contracted throats. But, by thus contracting the throat, the friction of the air is increased, and its velocity being also increased, the sound which is said to denote a strong "draught," follows, as a necessary consequence. The air being very much expanded from its increase in temperature, and its rapid escape in large volume, being prevented by the contraction of the throat, the contact with the combustible is not only prolonged, but the real quantity in contact, at any one time, may be supposed to be considerably diminished; yet, this being more frequent and rapid, the union is more perfect, and, consequently, more intense heat is produced.

The superior light of an Argand lamp, is, probably, in consequence of surrounding the burner with a glass chimney, by which the current of air is considerably elevated in its temperature, and the volume admitted is diminished, and not increased, as is generally supposed. Whether its increased velocity through the chimney is advantageous in the process of combustion, when abstractly considered, may be questionable; but, it is evidently advantageous in dissipating the products of combustion, or rather, imperfect combustion, which would

otherwise remain longer in contact with the flame. If the chimney be removed from the burner, the flame will be increased to double its former length, yet the light is pale, and the quantity emitted is much less. When the burner is surrounded by the glass chimney, if the wick remain at the same height, the strength of light required can be better regulated by the quantity of air admitted, than in any other manner; and for this purpose, these lamps should be furnished with delicate valves, and the most intense light will not be found, when the largest quantity of air is admitted.

The advantage of elevating the temperature of the air, is demonstrated by the increased intensity of light, which is produced by the button sometimes used in these lamps.

Common Names of Woods and Coals.	Botanical Names.	Specific Gravities of dry Wood.	Avoirdupois pounds of dry Wood in one cord.	Product of Charcoal from 100 parts of dry Wood by weight.	Specific Gravities of dry Coal.	Pounds of dry Coal in one bushel.	Pounds of Charcoal from one cord of dry Wood.	Bushels of Charcoal from one cord of dry Wood.	Time 10° of Heat were maintained in the room, by the combustion of one pound of each article.	Value of specified quantities of each article, compared with Shell-bark Hickory as the standard.
									H. M.	Cord.
WHITE ASH,	*Fraxinus americana,*	.772	3450	25.74	.547	28.78	888	31	6 40	77
APPLE TREE,	*Pyrus malus,*	.697	3115	25	.445	23.41	779	33	6 40	70
WHITE BEECH,	*Fagus sylvestris,*	.724	3236	19.62	.518	27.26	635	23	6	65
BLACK BIRCH,	*Betula lenta,*	.697	3115	19.40	.428	22.52	604	27	6	63
WHITE BIRCH,	*Betula populifolia,*	.530	2369	19	.364	19.15	450	24	6	48
BUTTER-NUT,	*Juglans cathartica,*	.567	2534	20.79	.237	12.47	527	42	6	51
RED CEDAR,	*Juniperus virginiana,*	.565	2525	24.72	.238	12.52	624	50	6 40	56
AMERICAN CHESNUT,	*Castanea vesca,*	.522	2333	25.29	.379	19.94	590	30	6 40	52
WILD CHERRY,	*Cerasus virginiana,*	.597	2668	21.70	.411	21.63	579	27	6 10	55
DOG WOOD,	*Cornus florida,*	.815	3643	21	.550	28.94	765	26	6 10	75
WHITE ELM,	*Ulmus americana,*	.580	2592	24.85	.357	18.79	644	34	6 40	58
SOUR GUM,	*Nyssa sylvatica,*	.703	3142	22.16	.400	21.05	696	33	6 20	67
SWEET GUM,	*Liquidambar styraciflua,*	.634	2834	19.69	.413	21.73	558	26	6	57
SHELL-BARK HICKORY,	*Juglans squamosa,*	1.000	4469	26.22	.625	32.89	1172	36	6 40	100
PIG-NUT HICKORY,	*Juglans porcina,*	.949	4241	25.22	.637	33.52	1070	32	6 40	95
RED-HEART HICKORY,	*Juglans laciniata?*	.829	3705	22.90	.509	26.78	848	32	6 30	81
WITCH-HAZEL,	*Hamamelis virginica,*	.784	3505	21.40	.368	19.36	750	39	6 10	72
AMERICAN HOLLY,	*Ilex opaca,*	.602	2691	22.77	.374	19.68	613	31	6 20	57
AMERICAN HORNBEAN,	*Carpinus americana,*	.720	3218	19	.455	23.94	611	25	6	65
MOUNTAIN LAUREL,	*Kalmia latifolia,*	.663	2963	24.02	.457	24.05	712	30	6 40	66
HARD MAPLE,	*Acer saccharinum,*	.644	2878	21.43	.431	22.68	617	27	6 10	60
SOFT MAPLE,	*Acer rubrum,*	.597	2668	20.64	.370	19.47	551	28	6	54
LARGE MAGNOLIA,	*Magnolia grandiflora,*	.605	2704	21.59	.406	21.36	584	27	6 10	56
CHESNUT WHITE OAK,	*Quercus prinus palustris,*	.885	3955	22.76	.481	25.31	900	36	6 30	86
WHITE OAK,	*Quercus alba,*	.855	3821	21.62	.401	21.10	826	39	6 20	81
SHELL-BARK WHITE OAK,	*Quercus obtusiloba?*	.775	3464	21.50	.437	22.99	745	32	6 20	74
BARREN SCRUB OAK,	*Quercus catesbæi,*	.747	3339	23.17	.392	20.63	774	38	6 30	73
PIN OAK,	*Quercus palustris,*	.747	3339	22.22	.436	22.94	742	32	6 20	71
SCRUB BLACK OAK,	*Quercus banisteri,*	.728	3254	23.80	.387	20.36	774	38	6 30	71
RED OAK,	*Quercus rubra,*	.728	3254	22.43	.400	21.05	630	30	6 20	69
BARREN OAK,	*Quercus ferruginea,*	.694	3102	22.37	.447	23.52	694	29	6 20	66
ROCK CHESNUT OAK,	*Quercus prinus monticola,*	.678	3030	20.86	.436	22.94	632	28	6	61
YELLOW OAK,	*Quercus prinus acuminata,*	.653	2919	21.60	.295	15.52	631	41	6 10	60
SPANISH OAK,	*Quercus falcata,*	.548	2449	22.95	.362	19.05	562	30	6 20	52
PERSIMON,	*Diospyros virginiana,*	.711	3178	23.44	.469	24.68	745	30	6 30	69

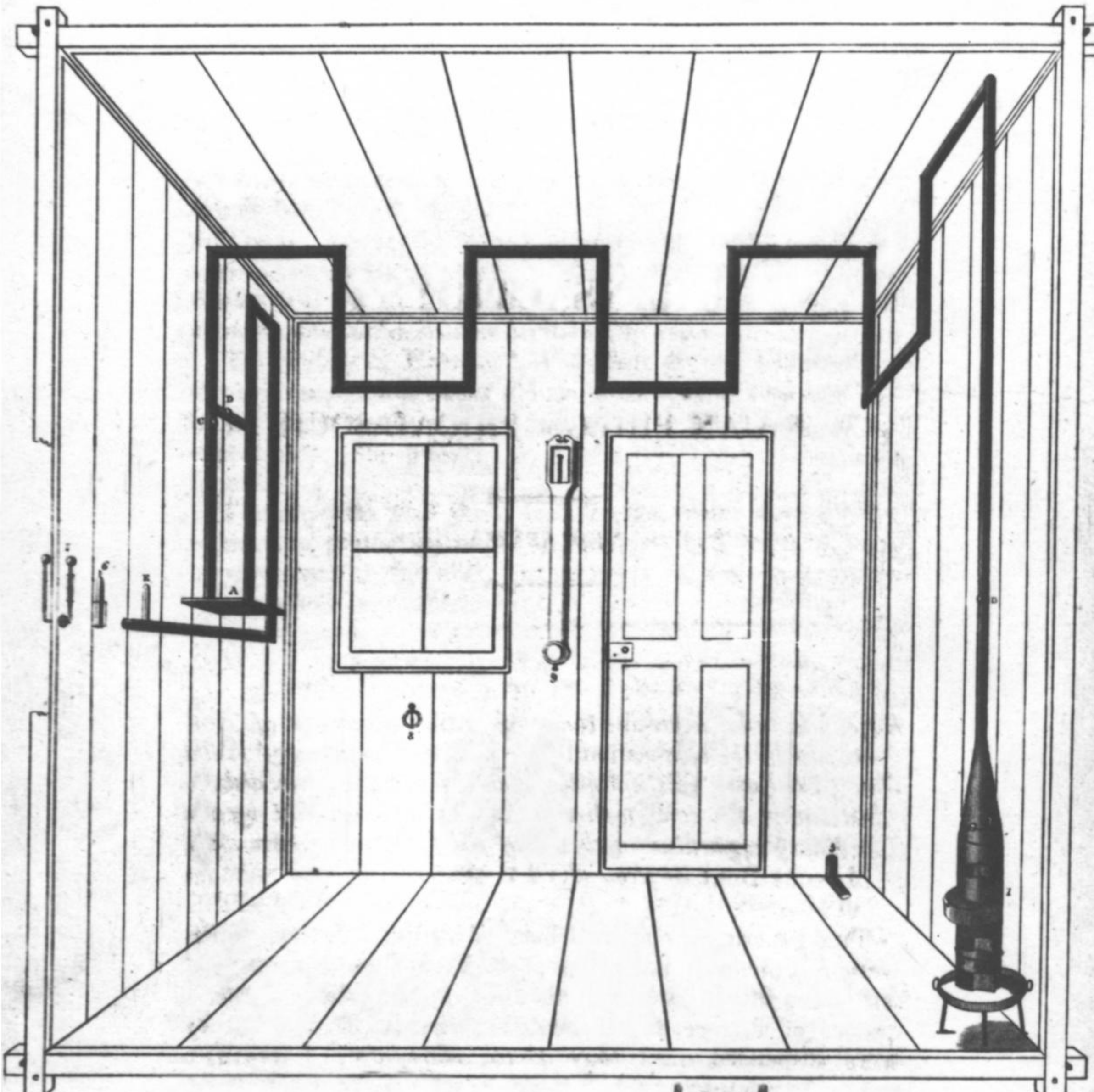
Common Names of Woods and Coals.	Botanical Names.	Specific Gravities of dry Wood.	Avoirdupois pounds of dry Wood in one cord.	Product of Charcoal from 100 parts of dry Wood, by weight.	Specific Gravities of dry Coal.	Pounds of dry Coal in one bushel.	Pounds of Charcoal from one cord of dry Wood.	Bushels of Charcoal from one cord of dry Wood.	Time 10° of Heat were maintained in the room, by the combustion of one pound of each article.	Value of specified quantities of each article, compared with Shell-bark Hickory as the standard.
									H. M.	Cord.
YELLOW PINE, (SOFT,) ...	*Pinus mitis,*	.551	2463	23.75	.333	17.52	585	33	6 30	54
JERSEY PINE, ...	*Pinus inops,*	.478	2137	24.88	.385	20.26	532	26	6 40	48
PITCH PINE, ...	*Pinus rigida,*	.426	1904	26.76	.298	15.68	510	33	6 40	43
WHITE PINE, ...	*Pinus strobus,*	.418	1868	24.35	.293	15.42	455	30	6 40	42
YELLOW POPLAR, ...	*Lyriodendron tulipifera,* .	.563	2516	21.81	.383	20.15	549	27	6 10	52
LOMBARDY POPLAR, ...	*Populus dilatata,*	.397	1774	25	.245	12.89	444	34	6 40	40
SASSAFRAS, ...	*Laurus sassafras,*	.618	2762	22.58	.427	22.47	624	28	6 20	59
WILD SERVICE, ...	*Aronia arborea,*	.887	3964	22.62	.594	31.26	897	29	6 20	84
SYCAMORE, ...	*Acer pseudo-platanus,* ..	.535	2391	23.60	.374	19.68	564	29	6 30	52
BLACK WALNUT, ...	*Juglans nigra,*	.681	3044	22.56	.418	22	687	31	6 20	65
SWAMP WHORTLE-BERRY,	*Vaccinium corymbosum,* .	.752	3361	23.30	.505	26.57	783	29	6 30	73
										Ton.
LEHIGH COAL, ...	....	..	..	..	1.494	78.61	..	..	13 10	99
LACKAWAXEN COAL, ...	....	..	..	..	1.400	73.67	..	..	13 10	99
RHODE-ISLAND COAL, ...	....	..	..	..	1.438	75.67	..	..	9 30	71
SCHUYLKILL COAL, ...	....	..	..	..	1.453	76.46	..	..	13 40	103
SUSQUEHANNA COAL, ...	....	..	..	..	1.373	72.25	..	..	13 10	99
SWATARA COAL, ...	....	..	..	..	1.459	76.77	..	..	11 20	85
WORCESTER COAL, ...	....	..	..	..	2.104	110.71	..	..	7 50	59
										100 Bushels.
CANNEL COAL, ...	....	..	..	..	1.240	65.25	..	..	10 30	230
LIVERPOOL COAL, ...	....	..	..	..	1.331	70.04	..	..	9 10	215
NEWCASTLE COAL, ...	....	..	..	..	1.204	63.35	..	..	9 20	198
SCOTCH COAL, ...	....	..	..	..	1.140	59.99	..	..	9 30	191
KARTHAUS COAL, ...	....	..	..	..	1.263	66.46	..	..	9 20	208
RICHMOND COAL, ...	....	..	..	..	1.246	65.56	..	..	9 20	205
STONY CREEK COAL, ...	....	..	..	..	1.396	73.46	..	..	9 50	243
HICKORY CHARCOAL, ...	....	..	..	..	.625	32.89	..	..	15	166
MAPLE CHARCOAL, ...	....	..	..	..	.431	22.68	..	..	15	114
OAK CHARCOAL, ...	....	..	..	..	.401	21.10	..	..	15	106
PINE CHARCOAL, ...	....	..	..	..	.285	15	..	..	15	75
COAK, ...	....	..	..	..	.557	29.31	..	..	12 50	126
COMPOSITION OF TWO PARTS LEHIGH COAL, ONE CHARCOAL, AND ONE CLAY, BY WEIGHT,	....	..	..	..	..	..	..	..	13 20	

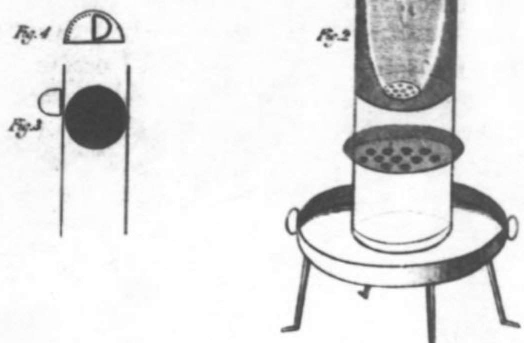
TABLE,

Exhibiting the results of experiments made to determine the comparative loss of heat sustained by using apparatus of different constructions, for the combustion of fuel.

No.	Description of Apparatus used.	Time the room was maintained at the same temperature in the combustion of equal weights of fuel compared with apparatus No. 9.	Weight of fuel required by each apparatus to maintain the room the same time and temperature compared with No. 9.
1	CHIMNEY FIRE-PLACE, of ordinary construction for burning Wood, ...	10	1000
2	OPEN PARLOUR GRATE, of ordinary construction for burning anthracite Coal, ...	18	555
3	OPEN FRANKLIN STOVE, with one elbow joint and 5 feet of six inch pipe placed vertically, the fire-place being closed with a fire-board, ...	37	270
4	CAST IRON TEN PLATE STOVE, with one elbow joint and five feet of four inch pipe placed horizontally, entering the fire-board, ...	45	222
5	SHEET IRON CYLINDER STOVE, the interior surface coated with clay lute, with one elbow joint and 5 feet of two inch pipe placed horizontally, entering the fire-board, ...	67	149
6	SHEET IRON CYLINDER STOVE, as before described, with three elbow-joints, 4½ feet, and 9 feet of two inch pipe, the whole placed as follows: 3½ feet horizontally, 5 feet vertically, for an ascending current, and 5 feet vertically for a descending current, entering the fire-board, . . .	78	128
7	SHEET IRON CYLINDER STOVE, as before described, with three elbow joints, 4½ feet, and 9 feet of two inch pipe, placed as follows: nine inches vertically and 12¾ feet horizontally entering the fire-board, ...	82	122
8	SHEET IRON CYLINDER STOVE, as before described, with nine elbow joints, measuring 13½ feet of two inch pipe, entering the fire-board, ...	95	105
9	SHEET IRON CYLINDER STOVE, as before described, with 42 feet ot two inch pipe, as used in the course of experiments on fuel, ...	100	100

APPARATUS USED BY M. BULL. IN HIS EXPERIMENTS ON FUEL.





Drawn by G. Strickland Engraved by C.G.Childs.